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Art. XLI.—*William Rowan Hamilton.*[1]

William Rowan Hamilton, one of the ablest mathematicians that this or any other country has produced, and for nearly forty years a fellow of the Royal Astronomical Society, was born in Dominick Street, Dublin, in the year 1805. His father was by profession an attorney, and was long held in great estimation both for his personal character and his professional ability. The branch of the Hamilton family from which he was descended originally settled in the north of Ireland, in the reign of James the First; and it is said that by right a baronetcy belonged to the representative of this branch, a near relative of his own; although the claim could not be fully supported, owing to merely technical flaws. Thus Hamilton may have been in some degree indebted for his great and versatile mental capacity to a mixture of race.

William Hamilton is one of those rare instances, where the promise of early childish precocity has not been disappointed by the attenuated achievements of riper years. At various stages of his boyhood, not to say childhood, for the precocity manifested itself at the early age of *four*, he is said to have successively acquired some notable acquaintance with no less than thirteen languages, European and Asiatic. His attention was directed to the latter, because it was originally hoped that, enjoying as he did the opportunity of good patronage, his career would be passed in India. It is recorded on evidence which deserves re-

[1] From the Monthly Notices of the Royal Astron. Soc., xxvi, 109.

spect, that at the age of seven he was examined in Hebrew by a Fellow of Trinity College, Dublin, and that "the child passed a better examination in that language than many candidates for the fellowship." For obvious reasons we hope there is some pardonable though very natural exaggeration in the statement. It is certain however that the attention of the Persian Ambassador, when on a visit to Dublin, was attracted by a letter of greeting written in Persian by young Hamilton at the age of fourteen. Whether or not any allowance is to be made for the shadow of the future overlapping the memory of the past, it is quite certain that the vast intellectual capacities of the boy were evinced and cultivated at a very early age, and what is of far greater consequence, this early mental activity did not prostrate or forestal the successful exertions of maturer life. It is quite possible that the literary turn thus given to his earlier pursuits may have happily laid the foundation of that peculiar combination of metaphysics and poetry, which distinguished some of his mathematical performances from those of most other men. For his early training in ancient and modern languages, he was indebted to the loyal care of his uncle, the Rev. James Hamilton, curate of Trim; but in science and mathematics he appears to have been nearly self-taught and self-directed; in his case, as in that of many other eminent men, this circumstance probably conduced to the originality of his maturer conceptions, and to the peculiar style in which he embodied them.

By the age of fifteen, young Hamilton had mastered the usual course of elementary mathematics, pure and applied; and in some instances had become familiar with works of original research. He appears to have evinced a peculiar taste for long and difficult arithmetical approximations, and to have shown himself no mean antagonist in the solution of numerical puzzles when matched against a certain arithmetical prodigy, who, coming from America, happened at that time to be exhibited in Dublin. By the age of seventeen he had mastered Newton's *Principia*, and a year later found him in possession of most of the processes in the *Mécanique Céleste*. Meanwhile, and notwithstanding this very unusual advancement in mathematical knowledge, the main culture of his mind had been classical; and that, not alone from natural predilection, but on account of the requirements of the collegiate course on which it was his intention to embark and to compete.

It is almost needless to say that young Hamilton, with a mind thus disciplined and furnished, entered upon his course at Trinity College, Dublin, if not without able competitors, at all events without an equal, whether in literature or mathematics. As might be expected, he carried all before him; and when we speak of success in his literary efforts, it must be understood

that we include Poetry in the list, inasmuch as on two successive occasions he gained the Vice-Chancellor's Prize for English verse. It is to this early and successful cultivation of the lighter elegancies of scholarship that his friends were indebted for a vein of poetical thought and expression which graced alike his correspondence and his conversation, and which is sometimes observable even in his graver compositions.

It appears that in the year 1822, one year before his entrance at the University, young Hamilton, now in his eighteenth year, attracted the notice of the celebrated Dr. Brinkley, by certain objections which he made to a demonstration propounded by Laplace in the *Mécanique Céleste.* On being invited to pay a visit to that well-known astronomer, the young student thought that he should most properly express his feelings of respect by carrying in his hands another instance of independent research on the osculation of certain curves of double curvature. This introduction of Hamilton to the veteran professor laid the foundation of a mutual friendship and respect which continued to increase during Dr. Brinkley's tenure of office.

In the first year of his student life at Dublin, Hamilton, notwithstanding his close attention to the elementary line of study necessarily prescribed to undergraduates, nevertheless engaged himself in a line of original research. Even before his entrance at the University he had directed his thoughts to the difficult subjects of Caustics, and having now completed the memoir, it was read before the Royal Irish Academy in 1824. This paper was referred as usual to the consideration of a committee of scientific men, who being struck with the originality of the conception, and the evidences of analytical power which it contained, recommended the author to give those further developments of the subject which evidently lay within his grasp. The result of this encouragement to the young philosopher was the speedy completion of a memoir which may be said to contain the germ of a large portion of the noble work which it was his lot to contribute toward the advancement of physical knowledge. Instead of an essay on Caustics, his paper was now enlarged into a wider and more general investigation, under the title of a "Theory of Systems of Rays." It may be no exaggeration to say of this memoir, in conjunction with its subsequent supplements, that it is one of the ablest contributions ever made to our knowledge of the geometry of optics. Chasles, one of the most distinguished of modern geometers, speaks of it as "*dominant toute cette vaste théorie.*" Starting from the simple fundamental principle that light, whatever may be its cause or its constitution, is amenable to what mathematicians call "The Principle of Least Action," or, in other words, probably as true, and certainly more expressive, amenable to the *principle of no waste*

in nature, Hamilton, in a train of analytical logic unimpeachable, and with a mastery over the management of algebraic symbols probably never surpassed, shows that the theory of a system of rays reflected or refracted any number of times at given surfaces, depends on the determination of a single principal function V, which contains in itself all the properties of the system of rays, in a manner analogous to that in which the properties of a curve are contained in its equation. The same theory is, in the supplements, extended to the more complicated and recondite question of double refraction in biaxial crystals, and at length lands the reader in one of the most remarkable scientific predictions contained in the records of physical inquiry. But of this prediction we must speak presently.

When the first part of this "Theory of Systems of Rays" was presented in April 1827 to the Royal Irish Academy, it will be remembered that Hamilton was as yet an undergraduate of twenty-one years of age. In this year the Professorship of Astronomy in Trinity College, Dublin, became vacant by the promotion of Dr. Brinkley to the bishopric of Cloyne. Such was his deserved reputation, that, notwithstanding the appearance of other and most formidable candidates in the field, and although, moreover, he had as yet taken no academical degree, Hamilton was elected to the vacant chair.

This circumstance is of itself sufficiently remarkable, and reflects equal honor upon the authorities who ventured to make the appointment, and on the young geometer who, by dint of genius and laborious study, was qualified to discharge the duties of the post. In connexion with this arrangement there is a point of osculation with our own Society of sufficient interest to demand our notice. The present Astronomer Royal, at that time Lucasian Professor of Mathematics in the University of Cambridge, was one of the candidates for the vacant post at Dublin; and he too, like Hamilton, had been advanced to his professorship before he had ceased nominally to be in pupilage. We are not here, even by the remotest implication, suggesting a comparison between these eminent men; such a comparison would not only be utterly unfitting, but, owing to the divergence of the lines of research adopted by the two geometers, would be wholly impossible. Nevertheless the thought unavoidably presents itself, that for both parties, and for the general interests of science, the decision of the Dublin electors was a happy one. Had it been otherwise, the one, in all probability, from certain natural tendencies of his mind, would have become a clergyman—no doubt a most eminent one—in the Irish Church; while Greenwich and our own Society might have lost the other.

> "There is a Divinity which shapes our ends,
> Rough hew them how we will."

In 1828 Hamilton became a Fellow of the Royal Astronomical Society, and thus at the time of his decease was among the oldest, as his name was certainly among the most honored, of our members. In 1833 he made known, in one of several supplements to the "Theory of Systems of Rays," his great discovery of Conical Refraction. In this memoir, starting again from the principle of least action, and, as before, conducting the investigation by means of a single Principal Function, he establishes the entire theory of double refraction; and, applying it to the case of biaxial crystals, by a new and simpler method[1] than that originally pursued by Fresnel, he obtains the equation to the form of the wave assumed by the vibrating ether within the crystal. On examining the form of the wave surface, Hamilton, with remarkable sagacity, observed that if the theory and the results were true, a single ray of light incident at a certain angle on a biaxial crystal, must of necessity pass into it, not as one ray, nor even as two rays, but as a conical sheet of light, and then finally emerge as a luminous cylindrical surface. And, again, his profound and complicated analysis indicated that there was also a direction within the crystal, such, that if an internal ray of light passed along it, it would emerge from the crystal, not as one ray, but as a luminous conical shell. Such results as these were not only apparently contrary to all analogy and expectation, but formed, if the experiment could indeed be made, a species of *experimentum crucis* of the truth of the undulatory theory of light. Notwithstanding the difficulty of the case, the experiment was at length successfully performed by Dr. Humphrey Lloyd, of Dublin, whose patient ingenuity, and faith in the profound work of the geometer, were rewarded by the sight, for the first time, of what cannot properly be called less than the astonishing phenomenon of a single ray spread out, by refraction in a crystal, into an infinite number of rays, forming the surface of a luminous cone.

From the sagacity of Hamilton and his friend Dr. Lloyd, thus constraining the little crystal of Aragonite to give up, Sphinx-like, its secret of ages, our thoughts unavoidably turn to the parallel case of Adams and Leverrier, who, from a similar strong faith in the laws of nature and in the logic of geometry, not only predicted the existence of a planet heretofore unseen and unexpected, but indicated the precise region of the heavens, where, as soon as looked for, it was actually found. We do not regard such results as valuable only because they corroborate our conviction of the existence of certain laws whereon we believe the universe to have been constructed by the Author of Nature, but

[1] It is but a point of justice to state that Mr. Archibald Smith has since much improved the simplicity of the process by a very elegant method of elimination.

still more so because they serve to encourage the student to persevere in his researches, animated by the fullest conviction that if truthfully conducted they can only land him in truth, and leaving the *cui bono* to be determined by the appreciations, or the wants, or the curiosities, of men in time to come.

The Royal Irish Academy took cognizance of Hamilton's great discovery, and of the profound mathematical skill whereby it was evolved, by conferring upon him their Cunninghame medal; and the Royal Society awarded him a similar mark of their appreciation of his merits. In 1837 he was elected President of the Royal Irish Academy, succeeding[2] his friend and early patron, Dr. Brinkley, in the chair, as he had succeeded him in the Professorship of Astronomy. He retained this distinguished office for eight years, and on his resignation he received the thanks of that eminent Academy "for his high and impartial bearing in the chair."

In 1834 and 1835 he communicated to the Royal Society two papers on "A General Method in Dynamics." Here, again, he commenced with the same fundamental idea, as that which he had already so successfully adopted in his "Theory of Systems of Rays," and he showed that the integration of the differential equations of motion for any system of bodies may be considered as depending on the determination of a certain Principal Function, which he defines in several different forms, but in each case by means of *two* partial differential equations involving, one of them, the differential coefficients in regard to the final coördinates, (coördinates at the time t), the other, those in regard to the initial coördinates of the several particles. He also established in these memoirs the now well-known "*Hamiltonian Form*" of the equations of motion of any material system.

The two Memoirs just referred to gave occasion to Jacobi's investigations on "Partial Differential Equations" (*Crelle*, t. xvii, 1837). Jacobi shows that, instead of "Hamilton's Function" involving the time and the initial and final coördinates, and satisfying *two* partial differential equations, it is allowable to consider a function of the time and the final coördinates only, satisfying a *single* partial differential equation: and he considers that by omitting to make this simplification, Hamilton presented his remarkable discovery in at least an imperfect light. There can be no doubt that the simplification thus introduced by Jacobi was a most important and valuable one; but it can scarcely be objected to Hamilton that he failed to perceive all the results deducible from his own discovery, any more than it can be objected to Fresnel that he left it to Hamilton to deduce conical refraction from the very form of the wave surface which Fresnel

[2] Dr. Lloyd, sen., was President for two years after the death of the Bishop of Cloyne. Hamilton succeeded Lloyd.

was the first to investigate. It must not be forgotten that it is to Hamilton's discovery as their fountain, though the course of the stream was directed by Jacobi, that are due all the developments which have since been made in the vast subject of Theoretical Dynamics. In a word, it may not be too much to say that the step in advance made by Hamilton's two memoirs can only be compared with that effected at an earlier epoch by the publication of Lagrange's *Mécanique Analytique*. For this work, also, Hamilton was again awarded a gold medal by the Royal Society.

We pass over various other characteristic works of this profound analyst, not because they are devoid of interest or of worth, but because they are less within the scope of our Society; and we come at lengh to what Hamilton considered the crowning labor of his life,—a labor which for the next twenty years, and indeed till within a few days of his decease, continued to occupy his thoughts. The labor here referred to was bestowed on the invention and the development of the Calculus of Quaternions. In a memoir such as this, and for the purposes which we have in view, we must almost despair of explaining, or perhaps of even conveying an idea of what is the aim and scope of the Calculus of Quaternions, or in fact what a Quaternion is; and yet without some such attempt, successful or not, any obituary notice of this great man would be incomplete. For this purpose, then, we must bear in mind that, in the method of geometry introduced by Descartes, and which has been retained in astronomical and physical investigations up to the present time, the position of a point in space has been determined either by its distances from three coördinate planes, or by what in reality are their equivalents. Hamilton, however, starts at once by considering not so much the position of a *point*, as rather the relation which exists between two *lines* intersecting in space, having regard both to length and to position. It will soon be seen that in order to determine these relations completely, four quantities, or four elements, are necessarily involved.

1. There is the relation which the length of the one line bears to the length of the other line;
2. The angle through which the one line must be conceived to be turned in order that it may coincide with the direction of the other;
3. The plane in which the two lines lie.

And inasmuch as the determination of this plane involves two elements, viz: 1st, its inclination to some fixed or known plane, and 2d, an element which is analogous to the longitude of a planet's node, it follows that four[3] elements or symbols are re-

[3] The above is in fact one of Hamilton's many illustrations of the meaning of a quarternion. Analytically speaking, a quarternion is an expression of the form $w+ix+jy+kz$, where i, j, k are imaginary roots of $\sqrt{-1}$, differing from the

quired to determine the relation which one line in space bears to another line.[4] The combination of these four elements, then, forms the Quaternion of Sir William Hamilton; and as handled and developed by him, these combinations unquestionably form a calculus of amazing generality, grasp and power. As an engine of investigation, in the general problem of combined rotations, the method of Quaternions probably has no rival in completeness or in facility. They remind one of the tentacles of some gigantic polype ramifying out into immensity, and bringing back with them the spoils of space.[5]

It is as yet premature to anticipate on which of his investigations or discoveries Hamilton's fame will ultimately rest. There are mathematicians among us who in this respect would be inclined to name his Calculus of Quarternions; others would say that none of his writings can overshadow the importance of his *Dynamical Theorems.* As yet, however, the former calculus can hardly be said to be fully developed, or to have been extensively applied by other philosophers to new lines of investigation; nevertheless, it can scarcely be supposed that the persistent and conscientious labor of such a man for twenty-two successive years can fail to be full of the seeds of thought, and one day be found to admit and to invite important applications. It must however be conceded that (partly perhaps on account of its comparative novelty, and partly on account of the metaphysical atmosphere which surrounds it), the method is neither easy nor attractive to any but the ablest and most daring of the analysts among us; many a man who has essayed to bend this bow has probably said to himself what Antinous said to his boon companions:—

> "Thou wast not born to bend
> The unpliant bow, or to direct the shaft."

imaginaries of ordinary algebra, in that the *order* of multiplication of these symbols is material, ij here not being $= ji$ but $= -ji$, and so for the other symbols.

The geometrical interpretation is this: on taking the usual three rectangular coördinate axes of x, y, z; if ij means rotate the axis of (y) round the axes of (x) through 90° of right-handed rotation, then ji must mean rotate the axis of (x) round the axis of (y) through 90° of right-handed rotation. Now the result of the former rotation is a line in the direction of the axis of $+z$; the result of the latter rotation is a line in the direction of the axis of $-z$; in this sense then $ij = -ji$ and so $jk = -kj$ and $ik = -ki$. The symbol (w) is the ratio of the lengths of two intersecting lines (or *vectors*) considered in the quaternion. Such is the first glimpse of this intricate Calculus.

[4] *Elements of Quaternions*, Longmans, 1866, page 110. This extraordinary work is the result of the unceasing labors of the last two years of Sir William Hamilton's life; indeed it is said to have been fatally injurious to his health. It was all but finished when the lamented death of the author arrested its entire completion. The Board of Trinity College, Dublin, have marked their sense of the value of this book by defraying the expenses of its publication.

[5] With this simile Sir W. Hamilton expressed his acquiescence to the writer of this memoir.

We have just spoken of the metaphysical atmosphere which seems to pervade Hamilton's Calculus of Quarternions; and herein there is little to excite our surprise, for it was natural for a man possessed of a mind so versatile and so profound, to turn it inward on itself; hence he delighted in metaphysics. But it was not alone because the culture and bias of his mind unavoidably led him in this direction, that many of his mathematical investigations assumed a metaphysical turn, but because he, in conjunction with other thoughtful philosophers, believed that no further great advance in mathematical science was now to be expected, excepting from the metaphysical point of view. Probably it is either a conscious conviction, or an intuitive perception of this, which influences the peculiar phase observable in the mathematical investigations of some of our greatest analysts of the present day.

Hamilton was not only a great mathematician, but by nature he was also a poet. He was heard to say, "I *live* by mathematics, but I *am* a poet." If, by this aphorism, he meant that, had he so chosen, he would have become more eminent as a poet than he is as an analyst, bystanders might hesitate to give their assent. Few men, perhaps, are fully conscious of the ruling bias and the strong points of their own minds. We know one of our greatest living philosophers who would perhaps say, "By filial duty I am an astronomer, but I was born a chemist." Of another it has been often said, "He *is* a mathematician and an observer, but he *was born* an engineer." Nevertheless Hamilton was a true poet, and by no means an indifferent writer of true poetry; and it is quite certain, that some of our subtlest mathematicians are poets at heart, knowing it and feeling it. And here it may be worth a passing remark to mention that Hamilton, in his great memoir on *A General Method in Dynamics* speaks of Lagrange's *Mécanique Analytique* as a *Poem*. One of our chief living Astronomers hereon remarks: "Hamilton was right, but he might have said a poem of most stately rhythm." The two works of Lagrange and Hamilton have points in common.

Hamilton counted among his friends, Coleridge, Southey, Mrs. Hemans, and Wordsworth. It is said that when Wordsworth through Hamilton's enthusiasm, was enabled to get a glimpse of the inexpressible fascination which surrounds the daring and *creative* spirit of modern geometry, the old man was for the first time inclined to admit even a mathematician into the charmed circle of the brotherhood of poets. The anecdote rests upon unquestionable authority; nevertheless we are inclined to think better things of so great and profound a mind as that of Wordsworth, and we are convinced that he must, by sheer dint of sympathy with other minds, have had at least a suspicion of the fact

before the great analyst revealed it. In vindication of the justness of these remarks on the expansiveness of great intellects, and on the poetic power which almost invariably is, at the least, latent within them, we cannot refrain from quoting the following sonnet, written by a great Astronomer, on the occasion of a visit to Ely Cathedral, in company with Sir William Hamilton :—

Sunday, July 29, 1845.

The organ's swell was hushed,—but soft and low
An echo more than music rang,—where he,
The doubly-gifted, poured forth whisp'ringly,
High-wrought and rich, his heart's exuberant flow,
Beneath that vast and vaulted canopy.
Plunging anon into the fathomless sea
Of thought, he dived where rarer treasures grow,
Gems of an unsunned warmth, and deeper glow.

Oh ! born for either sphere, whose soul can thrill
With all that Pöesy has soft or bright,
Or wield the sceptre of the sage at will,
(That mighty mace [7] which bursts its way to light),
Soar as thou wilt, or plunge,—thy ardent mind
Darts on—but cannot leave our love behind.

This memoir would be incomplete if we did not add, that our deceased member, together with the character of a scholar, a poet, a metaphysician, and a great analyst, combined that of a kind-hearted, simple-minded Christian gentleman; we say the latter because Sir William Hamilton was too sincere a man ever to disguise, though too diffident to obtrude, his profound conviction of the truth of revealed religion. Endued with such qualities as these, what wonder, if of his friends he was almost the idol, and of his university the pride; for he was gentle, and he was eloquent, and he spoke evil of no man, he defended the fair fame of the absent, and he held controversy with none.

Such then is an imperfect but unexaggerated sketch of this remarkable man. We will only add, that happily he did not live to survive himself, but in full possession of his faculties, almost in the very presence of the friends who had long admired him; and, what was no new thing to him, supported by the convictions and consolations of his faith, he resigned himself to his rest, as one who knew that he had done a work which had been given him to do. C. P. [8]

[7] The symbolic analysis of which the eminent and excellent individual (Sir W. R. H.) supposed to be addressed, has proved himself a most consummate master.—(*Essays by Sir John Herschel.*)

[8] In the preparation of this *éloge*, the writer has received much assistance from Dean Graves, P.R.I.A.; the Rev. R. P. Graves, of Dublin; and Professors De Morgan and Cayley.

ART. XLII.—*The Vowel Elements in Speech;* by SAMUEL PORTER, of Hartford, Conn.

[Concluded from page 189.]

THERE are certain *modes of action of the organs* in vowel utterance, which are to be noticed as the ground of some important properties and relations. It is observable that the open vowels (deg. 3), *it*, *end*, *at*, *up*, &c., tend, in general, to a quick, abrupt, explosive utterance,—the *i*, *é*, *e*, *ä*, and *ö* especially, and the others more or less,—except the *a* group, in which the same is true of the close vowel: they can be prolonged only by a considerable and rather unnatural effort, and then with abated force and a tendency to unsteadiness. The reason, as I conceive, is that they are formed by a neutral position of the tongue, neither drawn up toward the palate nor depressed from it: a position into which the tongue can be quickly thrown or jerked,—like thrusting an arm straight out from the body,—with firmness enough to serve for the instant for a strong vowel utterance. The reason holds peculiarly in the five anterior groups, in which the effect is most marked,—and as for the close *a*, its relationship to the open *ä* by similar position of the tongue has been already noticed.

As a matter of fact, one of these vowels is never lengthened without changing quality and becoming really another vowel: usually, either falling into the open-depressed degree,—as in the French *tête*, *fête*, from Latin *testa*, *festa*,—or sliding to a group just behind and to a closer degree,—as the prolongation of *met* may naturally give us *mére*, *care*. In the latter case, the tongue rises just back of the terminus of the vowel-tube and thus establishes a new terminus; in either case, the operation is perfectly natural on mechanical and physiological principles. The vowel in *tête*, &c., is unquestionably such as to be accounted for by one or the other of these processes; and the like is true of *près*, *accès*, &c.

The middle and close vowels, on the other hand, are incapable of the same abrupt, explosive quality; and, when prolonged, usually tend to become more close, or, when at the closest, to move forward into a contiguous or otherwise related vowel of another group. These effects, again, we ascribe to the peculiar mode of action of the tongue, as, after coming into line for the group, it has to be raised to the proper degree of closeness: it is like raising the arm a little way after extending it. This motion cannot well be suddenly and firmly arrested so as to produce an abrupt or explosive utterance. It is also more natural to continue this motion than to hold it arrested so as to prolong the vowel unchanged. Obviously, also, the effect of continuing

the impulse, after reaching the closest degree, would be to raise, or bend up, the tongue at a point further forward, and so to carry the vowel into another group. The middle may, however, sometimes take the course of the open degree, and move a step backward in becoming more close when prolonged.

The tendency of the open-depressed vowels, when prolonged, is, for like reasons, in the opposite direction: they incline to greater openness, so far as possible, or else to a backward movement. Thus, self, ten, &c. drawled into the open-depressed degree, incline to the *ä* if still further prolonged.

It is to be remarked, that the turn taken by vowels under change of quantity will be much influenced by the character of consonants succeeding.

These physiological actions and tendencies are important in their bearing upon vowel change in etymology, and as explaining the rationale of diphthongs and all compound vowel sounds. This will presently be illustrated by examples.

There are *relations of easy transition* between vowels of different groups,—fundamentally important as concerns the same matters just mentioned. These relations are not wholly determined by local position in the scale and on the diagram. It is in this as in geography. Localities contiguous on the map may, we know, be separated by impassable mountains, and others widely distant be in virtual proximity as united by channels of easy communication. Along with our map of the places of articulation, we need to take into view all the circumstances on which the relations among the several vowels depend.

A number of different series may be made out, founded on relations of easy transition. I will just indicate the most important, in a necessarily somewhat indefinite way, and merely in order to show that the principles I have laid down are the true physiological ground of established facts in the history of word-transformation. A full development of the application of the system would require that the original explorer should take it along with him into the field of philological research.

The two series, *a*, *ä*, *e*, *é*, *i*, and *a*, *å*, *o*, *u*, are of primary importance in philology. The *a*, *i*, and *u* are the primitive vowels of the Sanskrit and of the old Gothic, out of which the *e* and *o* were next developed,—the *ä* and *å*, as well as the *é*, not yet having a distinctly recognized separate existence. As founded on the order and manner of development in the Indo-European languages,—and the like appears, in fact, in the Semitic tongues,—we have the scheme of Jacob Grimm, with *a* at the apex of a triangle, *i* and *u* at the lower angles, and *e* and *o* respectively intermediate on the two sides. That is, we have the two series *a*, *e*, *i* and *a*, *o*, *u*. Physiologically, the first series moves forward on the line of the tongue, from the common point of de-

parture *a*,—through the relationship before pointed out between the *a* and *ä* groups; the other moves upward along the *velum palati*,—the position of the organs for *u* marking it plainly the natural terminus of a series. The first we may call *the lingual*, and the other *the back-palatal*, or *the guttural*, *series*. The vowels of the lingual series are also allied by the general direction of the vocal current forward, while in *å*, *o* and *u* it is upward;—the position of the tongue for this effect may be observed to influence the lower jaw: tending to protrusion in at least *å*, *o* and *u*, and to retraction in at least *a* and *ä*. The plausible and commonly accepted scheme which regards these two series as determined by the less and less palatal opening from *a* to *i* and the less and less labial opening from *a* to *u*, fails to suit the facts as they present themselves under accurate observation.

Other lines of vowel transition diverge from the guttural series forwards toward *i*. Thus, the open *å*, *o* and *u* are so related to the open *ö* by proximity and the direction of the vocal current as to run readily into that. The middle and close *ö* are so related to the *ä* on similar grounds; the same *ö* vowels, as falling between a middle or close *o* and an *i* or an *e*, make the German *ö* of the *umlaut;* and, by a similar process, we have the *ö* vowels in the French *eu* from an original *e*+*u*. The connection is intimate between all degrees of the *u* with the *i*,—the tongue being so placed for the *u* that, by raising the fore-part, it readily comes into position for the *i*. A similar operation takes place between *å* and *i* in the Eng. *oi* diphthong, and between several different vowels (*ö*[4] the proper one) and *i*, in the various ways of pronouncing the Eng. "long *i*." From the open vowels generally to the high position of the back tongue which forms the close or middle *u*, the transition is easy, at least in diphthongal combination, as will presently be exemplified. There is a special ground of transition between *i* and *u* in the similar positions of the soft-palate.

We are now prepared to consider the laws to which diphthongal combinations are subject; but I will first enumerate *the principal pure diphthongs that are possible.* They are:—

1. *a*+*i*:[14]—Eng. only in the word *ay*, or *aye*, or sometimes heard in Is*ai*ah, Sin*ai*, &c., and in the long *i*, wrongfully.
2. *å*+*i*:—t*oi*l, b*oy*; North of England long *i*.

[14] Instead of *i* non-labial as the final element, we may have, in each case, the labial *i* (Ger. *ü*, Fr. *u*). Dr. C. L. Merkel resolves the German diphthong *äu* (H*äu*ser, M*äu*se) and *eu* (F*eu*er, *Eu*le) into *a*+*ü*. His Physiologie der menschlichen Sprache (Leipsic, 1866, pp. 444) is an able and exhaustive treatise,—the author a thorough anatomist, and a careful and minutely exact as well as original investigator. Helmholtz and Brücke undergo the ordeal of sound and searching criticism at his hands.

3. $\ddot{o}^4+i^2$ (or, with the "glide,"[15] $\ddot{o}^4+\ddot{o}^3+i^2$):—long *i*, as h*i*gh, p*i*ne, &c.,—the Scotch long *i* is $\ddot{o}^3+i^2$, or $\ddot{o}^2+i^2$.
4. $\ddot{a}+i$:—an affected pronunciation of long *i*.
5. $e+i$:—"long *a*" (*a*le) with the vanish.
6. $a+u$:—improperly in *our*, &c.
7. $\mathring{a}+u$:—another wrong form of *our*, &c.
8. $o+u$:—"long *o*" (*o*ld) with the vanish.
9. $\ddot{o}^4+u^{2l}$ (with the glide, $\ddot{o}^4+u^3+u^{2l}$):—*our*, n*ow*, &c.;—the Scotch *our*, &c., are $\ddot{o}^{3l}+u^{2l}$.
10. $\ddot{a}^3$, or $\ddot{a}^4$, $+u$ (with glide, $\ddot{a}+\ddot{o}+u$):—Yankee *our*, n*ow*, &c.
11. e^4, or e^3, $+u$ (with $\ddot{o}$ glide):—ancient pronunciation of *few*, d*ew*, &c.; one form of Yankee n*ew*, r*u*de, sm*oo*th, &c.
12. i^4+u (with $\ddot{o}$ glide):—extreme form of Yankee n*ew*, r*u*de, sm*oo*th, d*o*, &c.
13. e^4+a:—Qu., A.-S. d*ea*f, c*ea*lf, &c.?
14. e^4+o:—Qu., A.-S. s*eo*fon, h*eo*fon, s*eo*lf, &c.?
15. u^4 or u^3+u^{1l}:—r*u*de, t*u*be, l*u*te, s*u*it, n*ew*, d*ew*, &c.

The relative quantity of the initial and final elements is not alike in all these; but is usually greater in the initial. Where I have omitted to mark the degree, there is more or less latitude of variation.

The fundamental law of the pure diphthongs is, that the combination be such that the tongue can be, and is, kept continuously tense in passing from the initial to the final element, and the lips in like manner in the case of labials. Accordingly, the movement is more usually in a forward direction, and may be at the same time from open to close;—simply from open to close in the same group, though common enough, is not usually regarded as a diphthong, though I have included one such (No. 15) in the list above; indeed, combinations from two adjoining groups have not always been reckoned as diphthongs,—the usual English long *a* and long *o*, for example. A movement which requires a relaxation of the tongue or lips in passing from one element to the other will interrupt the continuity of the vowel sound, and necessitate either a hiatus, or the intervention of a *y* or *w* consonant, making in the latter case an *impure diphthong*. Thus, $\mathring{a}+i$ (t*oi*l) is a forward movement, and gives continuous vowel sound: but the reverse, $i+\mathring{a}$, almost necessarily introduces a *y* sound, heard as in *yawl* ($i+y+\mathring{a}$). So we have $i+y+a$ in *ya*rd, and in the Italian p*ia*no, f*ia*mma, &c.; $i+y+\ddot{o}$ in *you*ng, milli*on*, billi*a*rd, &c.; $i+y+u$ in *u*nion, m*u*te, &c. A rearward movement can give a pure diphthong only when the first element, if not both, is quite open; as in Nos. 9 to 14 of the foregoing table. A continuous movement from close to open on the lips always introduces a consonantal *w*, as in the French

[15] A term used by phonetists, and denoting, strictly, the whole series intermediate, as the voice passes gradually from one position of the organs to another.

oui and *roi*, the Italian b*uo*no, and English q*ua*rter, *war* ($u^{1l}+w+\mathring{a}^{1l}$), *we* ($u^{2l}+w+i^{1}$).[16]

A word here on the subject of *pitch as related to diphthongal sounds*. I have remarked upon the tendency to rise or fall in pitch as the tongue moves forward or backward, or else as it rises or falls, on our vowel-scale. There are certain noticeable phenomena on which this has a bearing:—*first*, the rising inflection of a single tone on each syllable in ordinary unimpassioned utterance,—pointed out by Dr. Rush,—and *second*, that, in the change from the radical to the vanish of the vowel,—so prevalent in English,—and in all the usual diphthongs in short, the tongue-movement is either forward or upward. These facts, supposing the above-mentioned tendency to be real, have a plain mutual connection. Notice also, in impassioned utterance, that the rising inflection brings out the vanishing element more distinctly and more naturally than does the falling: compare *No?* or *Nay?* interrogative, with the same word in the positive tone of authority or of confident assertion. It may be worthy of inquiry, whether in those languages which are less diphthongal the level tone more prevails.

Let us now attend to some of the applications of the system in *accounting for etymological and orthoëpical changes*. A process which figures largely in the history of vowel-change is the *condensation of a diphthong into a simple vowel intermediate* between the extremes of the compound. Thus, in French, we have *ai* run together into an *e* or *ä* sound,—as in *ai*mer, j'*ai*, f*ai*re, ch*aî*ne,—and *au* into an *o*,—as in ch*au*d, c*au*se, p*au*vre,—and *eu* into an *ö*,—as in p*eu*r, v*eu*ve, j*eu*ne. That these digraphs were once really diphthongal in utterance, is quite certain. (See Dietz, Gram., vol. i.) Such change is common in most languages. In describing the *ä* vowels (p. 184, note), I adverted to the originally diphthongal character of *ai* and *ay* in English, as in pr*ai*se, v*ai*n, d*ay*, s*ay*, &c. So was it also with the *au* and *aw*, which now take an *å* sound, as in f*au*lt, c*au*se, dr*aw*, l*aw*. In the Sanskrit, we find the *e* and *o* sounds existing only as developed from *ai* and *au*;—the *e* and *o* are always and everywhere, at least in the Indo-European languages, secondary and derivative elements, owing their origin to this or to some other process of transformation. By the *umlaut*, which fills so large a place in modern German, we have, from *o*, from *a*, and from *u*, in the root, an *ö*, an *e* or *ä*, and a *ü*, developed as intermediate by the influence of an *i* originally existing—though afterwards

[16] Dr. Merkel, whose work comes into my hand as this article is passing through the press, approximates partially to the view presented above. He says, "The essence of the diphthong depends upon the convergence of the dilated vocal organs," and agrees with Brücke in ascribing the *w* and *y* to movement in the reverse direction, from close to open.

dropped or changed to *e*—in a succeeding syllable of inflection or derivation; as Wort–Wörter, Hand–Hände, kurtz–kürtzen; and as Bett, Ende, from old badi, andi;—relics of it in English are bed, end, men, sell (Goth. salyan), and other cases not a few; —and in the Old Norse, for instance, *a* takes umlaut from *u* in the next syllable. The umlaut is believed to have come up through an intervening stage of diphthongation,—badi, for example, becoming first baidi and then bedi or bed: but whether so or not, reference to it here is pertinent.

I maintain that such change is to be explained almost wholly by palato-lingual action. Of course it is so in the case of *e* or *ä* from *ai*. As for *au*, labial position would not determine its reduction to *o* or *å* rather than to a labial *ö*. In the *ü* from *ui* or *iu*, there is usually an admixture of a consonantal *y*, such as comes from a palato-lingual position between that for *u* and for *i*; and such may be presumed to have always appeared plainly in the first development of the *ü*. The labial quality is indeed imparted from the *u* to the *ü*. But, in Anglo-Saxon, the *y* which appears as umlaut of *u* was probably a non-labial; as the frequent interchange with *i* in writing would strongly indicate. The short *u* from which it arose was possibly non-labial, and the umlaut may have been distinguished from a quite open *u* or *i* by the consonantal *y* sound intermixed. Examples of this A.-S. *y* are byrig, mylen, cyrice, cymlic, cyning, byrden, bysig, bycgan, lystan, lytel, syn, ryne, (town, mill, church, comely, king, burden, busy, to buy, to desire, little, sin, running.)

The change above described comes rather as a natural result from the rapid utterance of the extremes in close succession than from an attempt to effect their simultaneous utterance. The developed product does not necessarily take the quality of either of the two extremes.

Change from a diphthong to one of its components, by dropping the other, is also not uncommon. Thus from A.-S. *ea*, we have shall, sharp, hard, calf, &c., as well as deaf, head, red, &c.; from A.-S. *eo*, seven, heaven, devil, &c.; and we have benefit, parish, venison, comparison, &c., from the old or the later French bienfait, paroisse, veneison, comparaison.

Another process of change is *from a simple vowel to a diphthong*,—commonly, if not always, effected by prefixing or annexing another simple vowel. Thus we have our "long *i*" ($\ddot{o}^4+i^2$) from a simple *i*,—in multitudes of words, of Anglo-Saxon, French, and Latin origin, such as wine, crime, time, drive, write, wife, wild, sight, idle, iron, &c.; and *ou* ($\ddot{o}^4+u^{2l}$) from *u*,—as house, mouse, hound, from A.-S. hus, mus, hund, &c. In French, we find *oi* from *i*, as boire, doigt, foi, moins, noir, voie, vois, from Latin bibere, digitus, fides, minus, niger, via, video, &c. The "guna" so common in Sanskrit is the strengthening of a vowel

by prefixing an *a*. In French, we have a short *i* prefixed, in b*i*en, br*i*ef, d*i*eu, f*i*el, f*i*er, p*i*ed, t*i*ent, &c., and an *e* before an *o* that is transformed to *u*, in f*eu*, j*eu*, n*eu*f, m*eu*le, p*eu*ple, h*eu*re, &c. (from f*o*cus, j*o*cus, n*o*vem, m*o*la, p*o*pulus, h*o*ra, &c.). In Italian, the *i* prefixed to *e* and *u* to *o*, form a marked feature: as f*ie*ro, s*ie*da, b*uo*no, &c. Examples of the new element suffixed we have in our "long *a*" and "long *o*" with the vanish;—in French, we have *i* added to *a* of the Latin, in cl*ai*r, *ai*mer, m*ai*n, &c.; *i* to *e* in O. Fr. m*ei*, tr*ei*s, l*ei*, v*ei*le, &c. (now moi, trois, loi, voile, &c.), and in the modern fr*ei*n, pl*ei*n, v*ei*ne, &c.; *i* to *o* in v*oi*x, conno*î*tre, &c., and to *u* in s*ui*s (s*u*m), &c.;—all of these digraphs having been once actual diphthongs. All such changes must accord with the laws of diphthongal combination, as before stated; to which may be added, that it is in converting a short vowel into a long one, and in giving greater quantity or weight to one already long, that the tendency to diphthongation is usually manifested.

From a diphthong thus produced, the original vowel may afterwards drop off, and through these two processes, a simple vowel may be replaced by another entirely different and perhaps quite remote on the scale. Thus, through Fr. br*i*ef, we have Eng. br*i*ef (with *e*, not *i*, silent), from Lat. brevis; f*i*erce, through f*i*er, or f*i*ers, from ferus; benef*i*t, through bienf*ai*t, from benefactum; g*o*vern, Fr. g*ou*verner, Lat. g*u*bernare; &c. So, in f*i*eld, f*i*end, p*i*erce, the superadded *i* is the only element now heard.

A change from one vowel to another may also take place by the roundabout process of *diphthongation and subsequent condensation* to an intermediate element.

A very important process of transformation is *when one vowel changes directly into another*, the two being nearly related by contiguity on the physiological scale, or by such modes of formation that the organs readily slide from one into the other. Sometimes it is a transition from one group to another, and sometimes a change of degree under the same group,—the routes from group to group being identical with the lines of easy transition above mentioned. Such transformation is often consequent on a change in quantity or in accentuation. Sometimes it is a strengthening and sometimes an attenuation; sometimes is promoted by influence of associated consonants, and sometimes by the assimilating force of a neighboring vowel. Very commonly it results from certain general tendencies prevailing in a language.

The changes of this sort in the *Italian as evolved from the Latin* are strikingly confirmatory of certain leading features of the scheme here set forth. The Italian exhibits a remarkable regularity in its development, having been little disturbed by outside influences,—and is thus, so far as it goes, peculiarly fitted

for the establishment of principles of phonetic change. There are recognized in Italian an *i* vowel, and a "close" and an "open" *e*,—so called,—the close (*chiuso*) being like the *e* of our scheme, or nearer to the *é*, and the open (*aperto*) either precisely or nearly like the *ä*. So there is a *u*, and a "close" and an "open" *o*,—the close nearer to the *u*, and the open probably nearer the *å*, than is the *o* in the scheme." Thus, *three* stand in order in what I called the lingual series, and *three* in the back-palatal, or guttural. Of each there is a long and a short; and these with the *a* are all the vowels of the language. As to their development from the Latin, the open *e*, says Dietz (Gram., i, 312 *et seq.*), arises (1) out of short *e*: as dea, bene, breve, &c.; (2) out of *e* in position: as ecco, bello, tempo, &c.; (3) out of *æ*: as Enea, preda, &c.;—the close *e* arises (1) from short *i*: as bevere, cenere (from bibere, cinis), &c.; (2) from *i* in position: secco, esso (siccus, ipse), &c.; (3) from long *e*: as arena, cera, &c.;—again, the open *o* comes (1) from short *o*: as bove, odio, opera, &c.; (2) from *o* in position: as fossa, pondo, orbo, &c.; (3) from *au* diphthong, also sometimes from long *o*;—the close *o*, (1) from short *u*: cova, doge, sopra (cubare, ducem, supra), &c.; (2) from *u* or *y* in position: dolce, molto, onda, torso, &c.; (3) from long *o*: onore, glorioso, &c.;—the long *i* and long *u* of the Latin remain, and the *a* is unchanged.

Now, if we suppose the Latin *e* to have been either the *e* of our scheme or nearer to the *é*, and the Latin *o* to have been the *o* or nearer the *u*, we find *the same rules in each set of these changes.* We have (1) short *i* changed to so-called "close *e*"—*é* or *e* of the scheme, and short *e* to nearly if not precisely our *ä* vowel; and again, short *u* to "close *o*," and short *o* to nearly if not fully our *å*;—and the resulting vowel is long by the Italian rules of quantity. That is, we have a long evolved from a short, together with a movement one station backward. All this accords with what was pointed out as a natural tendency physiologically considered: a short vowel is never a quite close one, and when lengthened tends mostly to greater closeness and often to transition backward. We have (2) *i*, in position, changed to "close *e*," and *e* to "open *e*;" and again *u* to "close *o*," and *o* to "open *o*." Here also we have the original vowel short, though in a syllable long by position; and though the vowel does not become technically long, the change is not improbably connected with a greater weight and predominance of the vowel sounds in modern Italian as compared with the Latin,—as appears in the frequent use of *ie* and *uo* in place of simpler vowels; or, the change may be ascribed to a general tendency to transition backward

[11] It may seem that one or two more vowel-places should be marked on the back-palatal part of our scale, since the ear can distinguish the sounds—though indeed only for the close or close and middle degrees: perhaps so many ought to be noted,—certainly if demanded by the exigencies of any single language.

prevailing in Italian,—as does the reverse in English. We have (3) an original long vowel or diphthong, with little or no change except the condensation of the diphthong,—just as ought to be expected in connection with the actual changes as above stated. Similar laws and tendencies have had partial sway in the other branches of the Latinic family.

Every language under the sun will show examples, in abundance, of changes, more or less regular, by direct transition from one vowel group into another,—whether we study it in its etymological history, its dialectic variations, or the mutations of orthoëpical fashion. *The changes in vowel-pronunciation which the English has undergone*, and for the most part within three hundred and fifty years, are many, though not all, of this description.

It should be received as an incontrovertible fact, that the vowels in English had once substantially the Latin and Italian sounds;—and this they had, indeed, for the most part, even to the early part of the sixteenth century. The long *u* had not then become diphthongal, as it now is. The long *i* was diphthongal; but probably as e^2+i^1. The long *e* and *ee* had then, a very few cases excepted, the proper *e* sound, heard still from the Irish in "*swate*," "*indade*," for *sweet*, *indeed*, and the like. Two hundred years ago, the *ee* had obtained its present pronunciation as in *eel*, but the long *e* single, and the *ea* and *ei* still retained substantially an *e* sound. The change of the long *a* has already been spoken of as having occurred since that time. The digraph *au* had its present pronunciation at the earliest of the two periods just mentioned; it was between these periods that the *oo* came to its present sound (*foot*, *food*) from that of long *o*. Two hundred years ago, the diphthongs *ou* and *oi* (*our*, *oil*, &c.) had the initial element not precisely as at present; the initial of *ew*, in *few*, &c., was more usually an *e*, instead of, as now, an *i* sound; while the first in *ai*, *ay*, was a proper Latin or Italian *a*, or wavered between this and an *ä*.[18]

[18] For a full exposition of one branch of this topic, see the article, *Shakespearean Pronunciation*, in the North American Review for April, 1864. In respect to the long *a* as in Shakespeare's time, the view there taken is not quite correct. A careful examination of Wallis and Wilkins leaves no doubt that they regarded it as identical with the Italian *a*. Dr. Wallis (Gram. Ling. Ang., 6th ed., 1765, p. 8) says of the English *a*, long and short: "Cambro-Brittani hoc sono solent suum *a* pronunciare; Italique suum;" and again (p. 55): "seu ut *a* Italorum." He describes it as the slender *a*, "*a* exile," in distinction from the broad *a*, "*â* pingue," used in the German and the French of that period, and heard in the English *all*, *hall*, *haul*, &c. Again, he describes the English *e* (pp. 9, 56) as like the *e* of the French, Spanish and Italians,—which identifies it, when long, substantially with our present "long *a*"; and he describes it further as intermediate between *a* and the *ee* in *feet*, or the *i* of the continent. His English *a* must clearly have had either a proper *a* or an *ä* sound;—also, the statement, in the same article, as to the *e* of that period, is seen to need correction. Bishop Wilkins, in his phonetic transcript of the Lord's Prayer and the Creed (Real Character, p. 373), employs one character for the *a* in *father*, *art*, *hallowed*, *name*, *as*, *day*, *daily*, *trespasses*, *temptation*, *and*, *Amen*, *maker*, *Mary*, *Pilate*, *was*, *again*, *at*, *hand*, *catholic*, *everlasting*, and for *o* in *body*. He uses an-

The point we have in hand,—that of direct transition from one vowel-group to another,—will be best illustrated *by taking into view, etymologically, the ancestral and allied tongues along with the English*, and adverting also to existing dialectic variations,—with this proviso, however, that the precise process of change cannot always be determined as direct rather than indirect and circuitous.

In the *a* group, we find a widely prevalent tendency to move in two separate directions: upon the lingual line to *ă* and still on to *e*, *é* and *i*, and upon the back-palatal line to *å* and yet beyond to *o* and *u*. Thus, from Sanskrit pad-as, we have, on the one hand, the Latin ped-is, and on the other, the Greek ποδ-ός, A.-S. fôt, Eng. foot; again, Sans. dant-as, Lat. dent-is, Gr. ὀδόντ-ος, A.-S. tôdh, Eng. tooth; Sans. ashtan, A.-S. eahta, Eng. eight, Lat. octo, Gr. ὀκτώ; Sans. naman, A.-S. nama, Eng. name (*e*), Lat. nomen, Gr. ὄνομα; Sans. jânu, Lat. genu, A.-S. kneo, Eng. knee, Gr. γόνυ; Sans. matri, Lat. mater, Gr. μήτηρ, A.-S. modor, Eng. mother (*ŏ*), Ger. mutter; Sans. mas, Gr. μήν, A.-S. mona, Eng. moon; and so on, indefinitely. So we have A.-S. bân, stân, hâm, &c., Eng. bone, stone, home, &c., Scottish bane (*e*), stane, hame, &c. The tendency of *a* to *å* is widely observable. The High German *a* is *å* in Low German dialects. The *a* in French vibrated into *å* in the seventh century, and so continued nearly through the eighteenth, when it swung back to *a*.[19] *Hand, stand, land, sand*, and the like, in the early English, we find most frequently spelled *honde, stonde, londe*, &c., even as late as Tyndale; and either this pronunciation was not universal or there was afterwards a return to the *a* sound. The Gothic had no long *a*, but a long *o* corresponding to the long *a* of the Sanskrit and employed whenever the law of etymological change would lengthen the short *a*. The sound was, undoubtedly, at first an *å*, but had come so near to *o* before the language was reduced to writing by Ulfilas, as to induce him to employ that letter. The long *o* in English comes but rarely from Anglo-Saxon long *o*, and frequently from A.-S. long *a*; we may, therefore, presume that it had not at first a proper long *o* sound, but

other for the *a* in almighty, and the *o* in Lord, of, for, from, &c.; and employs *e* for conceived, dead, Jesus, Amen, heaven, earth, &c.,—that is, for a sound which, when long, did not differ greatly, if at all, from the "long *a*" of our time. Reference to these two important authorities would have corrected the like error, or what I cannot but regard as such, on the part of Mr. Marsh, in his Lectures on the English Language, first series, Lect. xxii. I have not overlooked the noteworthy, but inconclusive, "Memoranda" of Mr. R. G. White, appended to vol. xii of his edition of Shakespeare.

For the earlier period referred to above, I have depended mainly upon Palsgrave, L'Eclaircissement de la Langue Française, the reprint by Genin, Paris, 1852 (1530 the original); for the other and later, my statements accord with those of Wilkins and Wallis. See also Atlantic Monthly, vol. iii, pp. 255, 6 (in the article on White's Shakespeare).

[19] See the article in the North American Review, referred to in the preceding note.

nearer to *å*; and the presumption is confirmed by the fact that the A.-S. long *o* became the English *oo*, and that this *oo* expressed the long *o* sound till at least the sixteenth century. The transition on this line is naturally from the long *a*; while the short *a* would move directly only on the other line, through *ä*: thus we have in Anglo-Saxon a short *ä* as the more frequent correspondent of Gothic short *a*,—as A.-S. *däg*, *mäy*, *ät*, *thät*, *bäd*, *gärs*, *gäf*, &c., for Gothic *dags*, *mag*, *at*, *thata*, *bath*, *gras*, *gaf*, &c. (Eng. day, may, at, that, bade, grass, gave, &c.).[20]

Attending now to *the lingual line*, we have no need to add to the instances already mentioned—our English "long *a*" among the rest—of gradual change from *a* through *ä* to *e*. From *e* on to *i*, examples are not less numerous. Thus, the *i* sound as expressed by *ee* in English, in *eel*, *deem*, &c., has arisen gradually by transition from the *e* sound, which the *ee* had in fact in early English,—and would have as replacing, for the most part, the A.-S. long *e*, while otherwise, with one or two exceptions, it is from A.-S. *ae*, *ea*, or *eo*. The same sound expressed by *ea*, *ei*, *ie*, as in h*ea*r, rec*ei*ve, bel*ie*ve, &c., whether from the French or the Anglo-Saxon, is almost always descended from an *e* sound. Many words are easily traced all the way from *a* to *i*: as str*ee*t, from the Latin str*a*ta, through the A.-S. strœte and strete; *E*ngland, with now an *i* sound, from *A*ngleland, through *Æ*ngleland and *E*ngland; p*ee*r, from Lat. p*a*r, O. Fr. p*ee*r, per; and agr*ee*, from Lat. ad gr*a*tum, Fr. agr*ee*r. In the Irish brogue, we notice the open *i* in place of open *e*: as r*i*nt, t*i*nant, g*i*ntlem*i*n, m*i*mber of parl*i*m*i*nt, &c. The tendency of the English has always been in a forward direction on this line, and on the others as well; yet sometimes we have the reverse movement, as in grass, f*a*ther, from A.-S. gräs, fäder, &c., and as our Southerners, some of them, both the white and the black, say *shore* for *sure*, *pore* for *poor*, and *star*, *har*, for *stair*, *hair*. The Yankee dialect goes beyond the common English in transition forward on the lingual line: as in g*i*t, y*i*s, *e*s, k*e*tch, for get, yes, as, catch, &c.; the same general tendency which leads to this inclines also to substitute the open non-labials, in the *u*, *o* and *å* groups, for the close labials, as requiring a less wide opening of the jaws, and in general less effort of the organs.

The French, as from the Latin, affords numerous examples of direct transition on the lingual line, in both a forward and a backward direction: as ch*e*r, s*e*l, t*e*l, from c*a*rus, s*a*l, t*a*lis; s*i*x, l*i*t, c*i*re, n*i*er, *aimé*, from s*e*x, l*e*ctus, c*e*ra, n*e*gare, *a*matus; m*oi*, cr*oi*s, av*oi*r, from m*e*, cr*e*do, habere; conse*il*, justesse, possède, verre, from cons*i*lium, just*i*tia, poss*i*deo, v*i*trum, &c.

On the back-palatal line, we find in English the *u* sound, in

[20] See the Grammars of Fiedler and of Mätzner for a full detail of letter-changes in the derivation of the English from the parent tongues.

words of Anglo-Saxon origin, most frequently from an A.-S. long *o*, as f*oo*t, g*oo*d, b*o*som, c*oo*l, st*oo*l, t*oo*th, d*o*, &c.; and as the A.-S. long *o* corresponds to the Gothic long *o*, and this to the Sanskrit long *a*, we have a regular precession from *a* to *u*. Examples in abundance to the same effect might be adduced from other languages.

On what we might call the *upper-palatal line*, from *u* to *i*, with only a consonantal *y* intervening, a familiar example of direct transition is the French *u* (i^l) from a Latin *u*,—most frequently from a long, though sometimes from a short *u*. The same change is found to have occurred in the Italian of Lombardy, and one similar took place at some period in the Greek; the Polish *y*, which is a non-labial, but corresponds etymologically to Sanskrit *u*, probably arose in a similar manner. The Scotch g*ui*d, or g*u*de, m*ui*r, s*u*ne, s*ui*ld, b*ui*k, (for g*oo*d, m*oo*r, s*oo*n, sh*ou*ld, b*oo*k,) &c., are noticeable in this connection. The vowels which spring from the *u* on this line, whether by direct transition or by the umlaut are somewhat diverse in themselves; but all admit of further precession or attenuation into an *i* non-labial and such as to betray no trace of origin from *u*. This has come to pass in dialects of Lombardian, French, Modern Greek, German, and other languages (see Dietz: Gram. I, 415); in English we have numerous cases of *i* from A.-S. *y* as umlaut of *u*,—as k*i*ng, s*i*n, k*i*n, br*i*dge, l*i*ttle, &c.

As for the transitional connection of the *ö* group with the others,—we may observe in the German *ö* and French *eu* somewhat of a proclivity to slide into a close *ä* in pronunciation. In our v*i*rtue, m*e*rcy, *ea*rth, &c. the vowels here followed by *r* have fallen back into an *ö* sound. The open *ö* has a near relation to the open vowels of several groups,—as its place on the physiological map would render probable. Transition into it is easy especially from *u*, *o* and *å*; and has taken place in s*u*n, b*u*t, *u*p, m*u*tton, in the vulgar t*u*ck for t*oo*k, s*u*t for s*oo*t, &c., and in s*o*n, d*o*ne, m*o*nkey, n*o*thing, m*o*ther, br*o*ther, &c. We know not whether the m*i*ther, br*i*ther of the Scotch are examples of *i* from *ö*, or from *o* by way of *u* in a form like the Ger. m*u*tter, br*u*der; but the latter would seem the more natural. From *a*, *e* and *i* to *ö* open, instances are numerous in unaccented syllables: as, liv*e*r, ov*e*r, robb*e*r, pill*a*r, doll*a*r, elix*i*r, nad*i*r, problem, pred*i*ct, tyr*a*nt, woman, Cub*a*, Missour*i*, def*i*nite, d*i*gest;—this before *r* is correct, but not approved in most other cases.

The *relations of certain vowels to certain of the consonants*, are well explained upon our scheme. Thus, the positions of the tongue for the open *ö* and for the usual English *r*,[21] requiring

[21] On the difference between the English and the continental *r*, as well as on other nice points in comparative phonology, see the very carefully and well prepared Introduction to the *Dictionary of the Noted Names of Fiction*, by William A. Wheeler, (Boston, 1865.)

but a slight change to pass from one to the other, favor the use of this vowel before the *r*,—as in cases just cited. Again, it often occurs that *l* gives way for a *u* in its place: as Fr. a*u*tre from a*l*ter, sa*u*f—sa*l*vus, fa*u*x—fa*l*sus, bea*u*—be*ll*us, chevaux for chevaⱡs, d*u*—de *l*e, a*u*—à *l*e, &c.; North-English a*w*most, a*w*d, for a*l*most, o*l*d; Scottish ha*u*d, ha*u*f, a*w*mous, sho*u*ther, &c. for ho*l*d, ha*l*f, a*l*ms, shou*l*der, &c. In the English *would, walk, talk,* &c. the *l* probably first gave way to a *u* sound which with the vowel preceding coalesced into one intermediate; and the *l* still heard in *altar, ball,* &c. probably had influence in the change from an *a* to an *a* sound. It sometimes occurs that *u* is prefixed, the *l* being retained, of which *would,* from *wolde,* must have been originally an example. Now, as *l* is not a labial but a lingual consonant, this relation cannot be accounted for at all, if we regard *u* as simply a labial, but is easily understood if we attend to the palato-lingual action, in the vowel as in the consonant. In forming the *l*, while the tip of the tongue touches the palate, the posterior portion also is raised and placed nearly as for the vowel *u*. Hence, when *l* is weakened by relaxation of the tip of the tongue, it naturally falls into *u* in certain cases,—and this occurs especially when coming after a vowel and before a consonant. But, in certain other cases, *l* passes into *i*: as Italian p*i*ano from p*l*anus, f*i*amma—f*l*amma, ch*i*aro—c*l*arus, p*i*acere—p*l*acere, ch*i*amare—c*l*amare, b*i*anco—Fr. b*l*anc—Eng. b*l*ank. Here *l* follows a consonant and precedes a vowel,—and the *i,* whose place reaches nearly to the tip of the tongue, results naturally from the weakened *l* in this case.

The frequent interchange between the vowel *u* and the consonants *w* and *v* is of course to be explained by reference to labial action.

In the case of the palatal, or gutturo-palatal, mutes, *k, g, ch,* (tenuis, media, asper,) with the related sibilants included, the variation induced by association with different vowels is altogether accordant with our scheme. Thus, in German, the difference of the *ch* in *nach* or *noch* and in *nicht* or *mich,* is too plain to be overlooked; and with the vowels throughout, this consonant articulation is more or less deep—near the throat, that is—in the order of our groups, *a, å, o, u, ö, ä, e, é, i.* So with *g* (hard) in English, the sound is really different, and certainly produced at a different place, in g*a*pe, g*o*ne, g*o*, g*oo*se, g*i*rl, g*a*rish, g*a*y, g*ea*r. In *Egypt,* we find it difficult, if we try, to give the hard sound at all to the *g,* coming thus between two vowels of the extreme anterior group. In words like *kind, card, guile, guard,* &c. a precession and partial softening of the consonant necessarily changes the succeeding vowel, in accordance with a quite prevalent style (Princ. of Pron. § 72, Note), either by interposing a vowel from a forward group, or, as may be done with

k*i*nd, g*ui*le, &c. by a mere precession of the initial of the diphthong: and, *v. v.* the change of the vowel would necessitate that of the consonant. Especially noticeable is a frequent change of Anglo-Saxon *g* into *u* or *w* and into *i* or *y* English, as determined by the antecedent vowel. Thus come m*aw*, dr*aw*, s*aw*, *ow*n, b*ow*, &c. from m*a*ga, dr*a*gan, s*a*ge, *â*gen, b*o*ga; and m*ai*n, m*ai*den, w*ai*n, s*ai*l, r*ai*n, l*ai*d, m*ay*, d*ay*, w*ay*, &c. from m*ä*gen, m*ä*gden, w*ä*gen, s*e*gel, r*e*gen, l*e*gede, m*äg*, d*äg*, w*eg*, &c. We have also m*au*dlin from M*ag*dalen. To explain fully the consonant gradation, it must be added that, while *k* and hard *g* always involve contact guttural, that is, of back-tongue with back-palate,[22] the point of contact may be higher or lower; it will be higher as the vowels associated advance forward on the scale; and, at the same time, the borders of the tongue will be applied to the palate in the precise place as for the vowel. A position will thus be reached which, omitting then the guttural contact, will give *y* for *g*, and German middle *ch* aspirate for *k*; or, replacing the guttural by forward contact, will give *g* soft, as *g*em, for *g* hard, and *ch* soft, as *ch*ill, *c* Italian, for *k* or hard *c*,—and, from these, transition is easy to Fr. *j*, or *z* in azure, to *sh*, or Fr. *ch*, and to *s*, or *c* in city. The vowel relations of the hard and soft, among these consonants, are to some extent familiar to all.

Finally, this view of the mode of formation of the vowels *gives the clue to the fundamental difference between vowel and consonant.* Of course the difference is plain enough in the case of the mutes *p*, *t*, *k*, and *b*, *d*, *g*, in which the sound is shut off from issuing through the mouth, as it is also in the nasals, *m*, *n*, *ng*. Again, in *f*, *th* (in *thin*), *sh*, *s*, &c. we have simply aspiration, or breath-sound, made at the articulating station, and in *v*, *th* (in *this*), *z*, &c. we have have the same mixed, or attended, with tone from the larynx; these all differ thus from the vowels, which consist of pure tone variously articulated. But we have *w*, *y*, *r*, *l*, which are, or at least may be, articulated with pure tone unmixed with breath sound. How, then, do these differ from the vowels?

Some phonologists hold that there is here no essential difference. There is a theory, strenuously advocated by Prof. W. D. Whitney, of Yale College, and by him first distinctly propounded, so far as I know, which regards the degree of open or close as the real and only ground of distinction between vowel and consonant,—what characterizes the vowels as such being their

[22] For *k* and *g* hard, the closure is always upon the soft-palate; made by the middle tongue and hard-palate it can give only an imperfect *t* and *d*,—Dr. Brücke's view to the contrary notwithstanding, for which see Prof. Packard's article, *Brücke's Physiology of Speech*, in the Bibliotheca Sacra for April, 1866, and which, it may be added, is fully disproved by the exact observations of Dr. Merkel,—though indeed the nature of the case, rightly considered, would render other disproof well-nigh superfluous.

openness, and the consonants, their closeness, while a neutral border-land lies between, neither decidedly one nor the other.[23]

Against this view,—which the well-merited reputation of Prof. Whitney as a phonologist entitles to very high respect,—it is to be observed that, in recognizing only a difference of degree—a difference of the same sort as exists among the vowels themselves—it fails to account for the marked difference in function between the two classes of elements. So that, if the contrast in degree of open and close be admitted as an actual fact, it is still not the material fact in the case. But the fact so assumed is far from being evident. Certainly, the *ch* aspirate is, in some cases at least, more open than some of the vowels; the usual English *r*, and still more the *r* made by trilling the uvula, is less close than the vowel i^1; and the consonant *w* and the vowel $u^{1\prime}$ may be so uttered—in the word *woo*, for example,—that the latter shall be closer, labially, than the former. As respects both *w* and *y*, more will be said presently on this point.

Resuming our inquiry, it is to be observed that *w* requires no special palato-lingual position, any more than does *v*; as let be tried on the word *way*. It is true that a lip-modification like what belongs to *w* may be taken by the vowel $u^{1\prime}$. This will make what may be called either an impure vowel or an impure consonant, and may fulfil the function of either a vowel or a consonant: of a consonant in a word like *we*, or *woe*, and of a vowel in one like *food*, or *good*, when uttered, of course, in the way supposed. A similar mixture of vowel i^1 with consonant *y* is not unusual, and some vowels may take a trill of the uvula as an accompaniment. But vowel and consonant quality are nevertheless distinct; and *w* as a consonant is not at all a palato-lingual articulation.

There remain *r*, *l*, and *y*: they are palato-lingual articulations; which are, or it is here admitted may be, uttered with pure tone; which also allow of indefinite prolongation;—agreeing in these respects with the vowels. But, to each of these, as to every consonant, there is wanting what we have had occasion to notice as a feature pertaining universally to the vowels,—the firmly-walled tube between tongue and palate, made by tension of the lingual muscles, with those of the soft-palate besides, so that the sound from the larynx is reverberated and reacted on as it passes through.[24] In the consonants just mentioned, there is merely

[23] See the two articles, by Prof. Whitney, *On Lepsius's Standard Alphabet*, in the Journal of the American Oriental Society, vols. vii and viii.

[24] The fact that each several vowel not only requires a special palato-lingual passage, but that this passage is a proper tube,—walled in all round by closure on each side,—is a point to which particular attention is desired, as it seems to have been hitherto unnoticed. The side teeth, which may be considered as forming a virtual extension of the arch of the hard-palate, are, it should be observed, a not unimportant auxiliary, as a sort of buttress against which the tongue may brace itself in position for the open anterior vowels.

presented a partial obstruction, of a yielding nature, over which the vocal current breaks, or by which it rubs, producing a murmur, burr, or trill, only, instead of a reverberation and ringing out of the sound.

Turning again now to the *w*, we find here a contrast of tension and non-tension of the lips as a further distinction between this consonant and the vowel *u*[11]. This may be made visible and palpable in the word *woo*, which presents the two elements quite open as labials to external observation.

In explaining the theory of the diphthong, there was occasion to notice the development of *w* between two vowels whose succession involves transition from a close to an open position of the lips,—as in the Fr. *oui* and the Eng. q*u*arter,—and of *y* in like manner in the transition from a forward to a backward (or what some would call a close and an open) palato-lingual articulation,—as in mill*i*on, bill*i*ard. Now, such transition requires a relaxation of the tension which characterizes vowel-articulation; and either a hiatus or a consonant must intervene. The simple relaxation of the articulating organs, with continuance of the tone, gives us *w* in one case and *y* in the other. From this we may conclude, first, negatively, that these consonants are not more close than the vowels from which they are developed by transition to a more open position; second, positively, that this relaxation, or non-tension, is what essentially distinguishes these consonants from the vowels.

It is admitted by Prof. Whitney (Jour. of Am. Orient. Soc., viii, 361–2) that *w* and *y*, as consonants, are sometimes more open than some of the vowels,—an admission which would seem difficult to reconcile with his theory. He says of them, also, "They are nothing but *i* and *u* themselves, deprived of the quantity and stress which belong to a full vowel utterance" and thus "assuming a consonantal value." But is it not plain that the ear discerns something positive in *y* and *w* which is not heard in *i* and *u*? Have we not, moreover, in the "vanish" in f*ai*l, *ai*sle, t*oi*l, s*ou*l, f*ou*l, an *i* or a *u*, deprived, to the utmost, of quantity and stress, yet making no approach to a *y* or a *w*? Quantity and stress are, neither of them, essential to a vowel; and a *y* or a *w* admits in fact of indefinite extension in quantity. The ready capability of the vowels for quantity and stress, we may well regard as due to the tension of the articulating organs rather than to their relative openness; and it does not, in the vowels, vary as the degree of their openness varies, for we have it admitted (ibid. p. 355) that the short vowels are more open than the long.

The *characteristic feature of the vowel* depends, then, upon *having a palato-lingual tube formed in the manner described for the reverberation of the sound from the larynx*, and consists in the actual

production and modification of the voice in that manner. When we add that, for the labials, the voice is further reverberated in a passage formed for such effect by the action of lips and cheeks,[25] we have stated all that pertains to the physiological definition of a vowel. But our view will be incomplete unless we see how it is that the vowel is thus fitted for its peculiar function in speech. It is possible to form a word or a syllable,—that is, a distinct, well-defined, vocal utterance is thus possible,—by a combination of mute and fluid consonants, or with a fluid consonant alone. *Pl*, *bl*, *cr*, *kn*, *prb*, *tlb*, *rl*, *sl*, *ll*, *rrr*, &c. are utterable without the help of a vowel;—but yet are never employed as words, though sometimes as dependent syllables. Why is it that a vowel, rather than a fluid consonant, is made a constituent part of every word? In the first place, the vowel gives to the voice a fullness, volume and force, a resonant quality, such as a fluid consonant can not. The voice finds, moreover, in the reaction it receives from the firmly braced yet unobstructive organs, a support on which it can lean, as it were, while still going out with free vent and full power. Upon the vowel, therefore, the main stress of the word is made to fall; and upon it the consonant elements hang in a relation of dependence, uniting with it easily and naturally as they do, whether prefixed or annexed, or both. Again, the vowel serves, by the stress and force it naturally takes, to mark off each separate word in connected speech,—that is, to give to each its unity and individuality as a word,—as must have been plainly the case in the original monosyllabic form. The superadded consonants render possible that variety in words which the ends of language require, and complete that capacity of fit expressiveness, in the word as related to the idea, which must once have been conspicuous, and of which all the traces are not yet absolutely obliterated. Thus it is that vowel and consonant concur to the bringing forth of the word. A few such elements, which take their character severally from processes of articulation, make up, by their varieties of combination, the whole of the outward form, or body, of that divinely ordained product of human instinct and intellect which we call speech or language, and which, in its various relations, presents one of the most interesting, and certainly not least important, subjects of scientific study.

[25] The fact is, that the want of resisting power in the lips, as against the vocal current, disqualifies them from acting alone in the articulation of a vowel,—limits them to the subordinate and secondarily modifying agency as above and before described. That the palato-lingual is really the primary agency in the labial vowels, may be readily proved by pronouncing, as can be done easily and with perfect distinctness, an *ö*, an *u*, an *o*, and an *ä*, with one and the same lip-modification for each and all.

ART. XLIII.—*Conclusive proofs of the animality of the ciliate Sponges, and of their affinities with the* Infusoria Flagellata; by H. JAMES-CLARK, A.B., B.S.

BEFORE I proceed to the main point in question in this article, I wish to say a word in regard to the group of animals, viz: the *Protozoa*, of which I am fully convinced the *Spongiæ Ciliatæ* are a part.

From the time when Ehrenberg published his great work, the "*Infusionsthierchen*," to the period of the issue of the "*Études sur les Infusoires*," of Claparède and Lachman, there has been a steadily growing belief that a large part of that mass of animalcules which Ehrenberg denominated *Infusoria* forms a distinct grand division, equally as decided in character as any of the four great groups which are now generally accepted. Still it is a little curious that, although Cuvier had long ago pointed out the *plan* or type upon which his four *embranchements* were constructed, later investigators have not attempted to elucidate the *typical idea* which lies at the basis of the Protozoan organization. Claparède and Lachman have approximated nearest to such an attempt in their division of a part of the group into dexiotropic and læotropic sections, but nothing is said, even there, of a plan which runs through the whole grand division. Surely they had seen enough of material—at least of the higher divisions of the group—to sustain them in pronouncing upon the typical relation of the Infusorian organization; but it may be that the apparent paucity of characters among the lower members of this grand division misled them into an apprehension that there was no definite taxonomical relationship of the organs. That they recognized the latter as members of the same group with the former no one will deny, but it must be conceded that the affiliation was observed to be only one arising from similarity of organization and habit, and not from any community of *plan* in the disposition of the organs.

It is now over two years since I demonstrated—in a course of lectures, delivered in February and March, 1864, at the Lowell Institute in Boston—that the arrangement of the organization of the *Protozoa* is based upon a spiral, or rather a helix: more recently those lectures have been published[1] and the *type* of the organization of the *Protozoa*, as well as that of each of the other four grand divisions of animals, made as clear, by illustrations, as the limits of the volume seemed to allow. In order therefore that I may not appear to claim for the Sponges merely a new position in the universe of obscurities, I shall take the liberty of

[1] H. James-Clark: "*Mind in Nature.*" D. Appleton & Co., New York, 1865.

drawing the reader's particular attention to the arguments which I have adduced—in the volume above mentioned—to prove the unity of plan in the organization of the *Protozoa*, and its dissimilarity from any other which dominates among the four remaining grand divisions.

This much being premised, I proceed now to give a sketch of the peculiarities of some of the genera of *Infusoria Flagellata* with which I think the Sponges are most intimately associated. Several of these genera are new to science, and moreover of the most remarkable forms. I regret that words alone cannot, at this time, render their peculiarities as evident as I hope the illustrations will, in my forthcoming paper, in the Memoirs of the Boston Society of Natural History.

I must ask the reader in the first place to go back with me almost to the Ultima Thule of animal simplicity, and revise the organization of the hitherto too lowly estimated *Monas*, in order to lay the foundation for the group which embraces in its limits so gigantic a family as the *Spongiæ Ciliatæ*. I do not think any one will be prepared to fully appreciate such a remarkable definiteness and system in the arrangement of the organization of *Monas* as I have discovered among the various forms which constitute that genus.

Hitherto a *Monas* has been looked upon as a mere shapeless molecule, with a vibrating *cilium* of some sort or other, attached to its surface at an indefinite point. As I understand the relation of parts, now, the *motory cilium* or *flagellum* is perhaps the most remarkable feature of the whole animal, not only in a physiological aspect, but also in its topographical relationship. Let me illustrate this by a description of the body and appendages of *Monas termo* Ehr.

The body of that species has the form of a wide, compressed heart, with two distinct summits. The broad flattened sides lie opposite to each other, and parallel with the plane which passes through the two summits, and which forms the prolongation of the greater transverse diameter of the body. Between these summits is an aperture which constitutes the mouth. One of the summits is prolonged into a broad, conical, beak-like body, and assists the mouth in the prehension of food. It is therefore a true *lip*. The *flagellum*, however, is the real prehensory organ, although it, at the same time, performs the office of a propelling agent, when the body is detached from its pedicel and moves about in a free state. This organ has the form of a scarcely tapering bristle, which is attached close to the edge of the mouth, on that side of it which is opposite to the lip, and rises with a decided, well-defined curve whose plane is coincident with the plane which runs through the two summits, and forms, as I have just mentioned, the plane of the greater transverse diameter of

the body. This remarkable feature is scarcely to be recognized during the free state of the animal; but when the latter is moored by its posterior end to its pedicel, the phenomenon in question is very marked and conspicuous. For most of the time the *flagellum* sustains itself in this rigid, arcuate position, and is always curved away from the lip; but its terminal end keeps up an almost incessant spasmodic incurvation toward the mouth, to all appearances for the purpose of throwing, or jerking, particles of food in that direction. When an acceptable morsel is met with, both the lip and the *flagellum* combine to press it into the open jaws of the animal; and when that is accomplished, the two organs immediately return to their former positions.

Scarcely less noticeable is the so-called *contractile vesicle*—the analogue of the heart of the higher animals. In a view of the body, so placed that the lip is next the eye, and the *flagellum* consequently curving away from the observer, we have the two broad sides on the right and left, and the plane of the greater transverse diameter coincident with the line of vision. The body then seems, at first sight, to have a symmetrical aspect, such as is not observable from any other point of view; and such it might be made to appear if I should belittle the importance of one single organ, by simply mentioning its existence, and omitting to lay down its exact topographical relationship. I refer to the *contractile vesicle*. During the systole of this organ it is so inconspicuous that it would easily escape even the most careful observation; but during the transition to the expanded state, and at the full diastole, its prominence, from the point of view just mentioned, is so great as to rival the *flagellum* in attraction. It may then be seen as a comparatively large, rounded, transparent, vesicular body, which stands out in strong profile, just in front of the middle, and close to the surface of the left side of the body. At full diastole it even forces the overlying region outwardly into a quite prominent papilla. In reference to the other organs and parts of the body, it stands, therefore, altogether in an asymmetrical relation; and from whatever point of view it—or any of the organs—may be observed, the organization as a whole evidently rests upon an oblique basis. The *bilaterality* of the type is sufficiently clear, but the topographical relationship of the organs is incompatible with *bisymmetry;* for right and left are twisted upon each other.

So much for *Monas*. As for the objection which has been raised against the estimate that has been put upon the monad-like Infusoria, because they have not been proved to be adult forms, it seems to me that the *onus* of proof lies on the other side, viz: to show that they are not adult. I think moreover that I am fully warranted in assuming that a *Monas* which possesses such an organization as I have described, and is attached

to a stem, is an adult; and more especially so since, among many hundreds which I have observed from time to time, I have never seen any trace of a transition to a higher form. That such simple organizations can exist without rising to a more complicated state, during a whole lifetime, I am furthermore sustained in believing by the discovery of some new generic forms, which, although scarcely, if at all, more highly organized than *Monas*, have in addition such characters as would seem to stand in the way of a transition to a more elevated grade of existence. For instance, the presence of a *calyx* about the body of an infusorian, into which it can retreat, is an indication of a fixity of condition which corresponds to the adult state. Thus I found one of the new genera which I just alluded to.

Bicosœca, as it is called, may be described in general terms as a stemless *Monas* which is attached to the bottom of a calyx, by a highly muscular, retractile cord. All the organs have the same remarkable definiteness of relationship and peculiarity of form that *Monas* possesses; and in addition there is the muscular cord which, with oft-repeated jerks retracts the body to the very bottom of the calycine envelope. There are two singularly diverse species of this genus; one marine and the other lacustrine.

The most interesting infusorian of this group of new forms is the one which I have called *Codosiga*. This links the Sponges to the flagellate Infusoria. Its greatest peculiarity consists in the possession of a highly flexible, extensible and retractile, membranous collar, or hollow cylinder, which projects from the anterior end of the body. The cylinder is slightly flaring, and, if we include the asymmetrical body, the whole might be compared to a very deep, one-sided bell, with its narrower end half filled up. The single, sigmoid-arcuate, rigid *flagellum* arises from the depths of the bell, exactly at the middle of the truncate front, as it were forming a prolongation of the longitudinal axis of the body. There is no lip, and the *flagellum*, which rises close to the mouth, has a strong resemblance to that of *Monas*, both in proportion, form, and habits; and performs the office of a prehensile organ when the body is fixed, or acts as a propeller during natation. The *contractile vesicles* are two, or even three, in number, and lie in the posterior third of the body. The only species of this genus which I know of is gregarious in habit, but usually not more than four or five bodies are to be found attached, like *Anthophysa*, by their narrower, posterior ends, to the branchlets of a single forking stem. The peculiarity in regard to the direction of the curvature of the *flagella* in a backward direction, toward the stem, is as highly marked in *Codosiga* as in *Anthophysa* (described by me in the September number of this Journal) and there is also the same fixed relationship of the longitudinal and the greater and less transverse diameters of the several individuals of the colony.

There is still another new genus which I would like to mention here because it forms a collateral link with *Codosiga* in the affiliation of the Sponges with the *Monadina*. This genus I have called *Salpingœca*. It is, as it were, a single individual of *Codosiga* which does not possess a stem, but is seated in a *calyx*, from which it protrudes, or into which it retracts, at will. There are three well marked species, of which one is marine.

I come now to the principal object of this communication. The sponge which formed the main basis of these investigations is the well known marine species *Leucosolenia* (*Grantia*) *botryoides* Bowerbank. It is preëminently a branching form, and, on account of the slenderness and transparency of its tapering, hollow ramules, is a most desirable object for study. A branchlet—and in fact the whole colony—may be stated to be essentially a double tube. The outer tube consists of a glairy, gelatiniform stratum in which the spicules are imbedded in a certain order, and is pierced by numerous ostioles, which are continued through the interior tube to its hollow center. The inner layer, or tube, is entirely made up of the individual members of the colony, the bodies of which are packed together closely, side by side like pavement stones, with their posterior ends slightly imbedded in the glairy substance of the outer tube, and their anterior ends projecting freely into the general cavity. To describe the shape and organization of one of these individuals would be to repeat, almost word for word, what I have already said of the monad of *Codosiga;* in short *Leucosolenia* bears some such sort of relationship to *Codosiga* that *Salpingœca* does; the latter being as it were a stemless *Codosiga* seated in a calyx, whilst *Leucosolenia* is comparable to a stratum of the monads of *Codosiga* imbedded in a spiculiferous envelope. It is clear therefore that the organic difference between *Leucosolenia* and *Codosiga* is scarcely enough to locate them in two different families; in fact I am inclined to consider them only as generically distinct, and hardly, if at all, more widely separated in this respect than are *Salpingœca* and *Codosiga*.

What are the diversities of other genera of the *Spongiæ Ciliatæ* I cannot more than conjecture; but seeing that one of the genera is so closely related to the monociliate *Flagellata*, it can hardly be possible that the others are very far removed, and I shall feel warranted, therefore, in assuming, upon the premises, that the whole group of *Spongiæ Ciliatæ* is as intimately allied with the monociliate *Infusoria Flagellata* as is possible for it to be without actually constituting, with the latter, a uniform family.

ART. XLIV.—*Caricography;* by Prof. C. DEWEY.

Continued from vol. xlii, p. 331, 1866. (No. 44.)

Index to the Species.

THE description of the species of Carex of our country in this Journal was especially designed to aid those who had just entered upon the study of our plants. It was begun in 1824, only seven years after the great impulse, produced by the lectures of Professor Amos Eaton, to the study of botany and some other branches of natural history. There were few works on botany accessible to students; and, even when I had in 1815 ascertained the principles of the Artificial System of Linnæus and studied the genera in a general botany, no work describing the species was accessible. The Gramina of Dr. Muhlenberg was published in 1817, and his Catalogue of our plants a little earlier. These, with Persoon's Synopsis, the Reedgrass of Christian Schkuhr, and a Botanical Dictionary, were the works of which Eaton made such valuable and extensive application. The standard authors on Carices then were Schkuhr and Muhlenberg; and they were implicitly followed, even in the few mistakes they made, and for the correction of which no method occurred for many years. As knowledge of the species increased, the difficulties became perplexing, especially in relation to *Carex crinita* and *C. paleacea*, *C. oligocarpa* and *C. digitalis*, *C. folliculata* and var. *xanthophysa*, *C. plantaginea* and *C. anceps*. While American botanists have solved many of the difficulties, far more has been effected in the study of our North American Carices by Francis Boott, M.D., of the Linnæan Society, England, who enjoyed the greatest facilities and showed the most persevering activity.

In the Index, the quoted authorities are necessarily few. The name of the species, or the *first* name (when more are given), is considered the designation *due* to the species; the synonyms are in *italics*. The references, besides Schkuhr and Muhlenberg already mentioned, are chiefly the following:

Monograph of N. Am. Cyperacea, by John Torrey, M.D., published in 1838, and his Botany of the State of New York, 1843. The reference is Mon. 1836, or Tor. 1836 or 1843, or both.

Flora Boreali-Americana, Michaux, 1803. Though earlier published, it was of little use to us, till the result of Dr. Torrey's Examination of the Herbarium of Michaux in Paris, was published in vol. xxvii, 1835.

Carices Am. Septen. Exsiccatæ; edidit H. P. Sartwell, M.D., 1848. Pars I et II. This collection of Carices, not figures, 158 in number, nearly all correct, and fine specimens, is very interesting. The reference is Sart. or Exsic. 1848.

Prof. Tuckerman's Enumeratio Methodica, 1843, scientific, discriminating and curious, has some important references.

The reference, Carey 1848, is to the species of Carex described by Mr. Carey, in Dr. Gray's Manual of Botany, 1st ed., 1848.

Dr. Boott's publications were more numerous. 1. In Hooker's Flora Bor.-Americana, 1840; 2. On a species of Carex, allied, &c. Trans. Lin. Soc., 1843; 3. Six new N. Am. species of Carex, in Boston Journal of Nat. Hist., 1845; 4. De Caricibus, in Hooker's Journal of Botany, 1846; 5. Caricis species Novæ vel minus Cognitae, Lin. Soc., 1851; 6. Illustrations of the Genus Carex, Part I. 1858; Part II, 1860; and Part III, 1862. These are severally quoted as Boott 1840, Boott 1851, &c. Boott's Illustrations of Carex is a magnificent work containing full descriptions and splendid figures of 291 species on 126 pages and 411 plates, forming three large folio volumes. It is proper to add that Part IV was nearly complete for the press, when this admirable man and botanist was removed by death, on Christmas morning, 1864: born in Boston, 1792, and died in London. With noble generosity too he distributed the "Illustrations" among his numerous botanical friends.

Figures of 119 species (numbered 117) are given in several volumes, ending vol. xlix, 1845. There are in vol. ix, 12 figures; vol. x, 11 figures; xi, 26 figs.; xii, 2; xiv, 5; xxv, 3; xxvi, 2; xxviii, 6; xxix, 18; xxx, 9; xliii, 4; xlviii, 8; and xlix, 12 figures. Many of these are finely characteristic; but want of space prevents more particular reference.

The following notices are important:

1. A Catalogue of one hundred and twenty-eight species in vol. xi, pp. 319–325, 1826, as then understood. Due attention was not paid to *priority* of names.

2. Species of Carex in the Herbarium of Dr. Muhlenberg, as named by him, in possession of the Am. Phil. Soc., Philadelphia, compared with the species then current, vol. xxv, pp. 142–6, 1834.

3. Carices collected in Arctic America by the English Explorers, with the current names; vol. xxvii, pp. 270–5, 1835. Some changes are to be found in the index.

4. Determination by Dr. Torrey of the Carices in the Herbarium of Michaux at Paris, vol. xxvii, p. 276, 1835. The two or three mistakes, resulting from others in part at least, were corrected by him in 1843 or by others afterwards; and the changes are found in the present volume, some in the index.

5. Some notices of *varieties*, in the forty-four papers of thirty-six volumes, are not in the index; and some of the earlier synonyms, as then understood, may not all have been corrected.

Finally. These results are respectfully submitted to the lovers of this difficult genus and its multitude of species. The additions and changes time will unfold. To the six hundred species (or more) of Carex, many more will probably be added in the coming twenty years.

INDEX.

Carex adusta, Boott 1840 (not Carey).
— *argyrantha*, Tuckerman,[1] 1859; this Jour., vol. xxix, p. 346, 1860; xli, 331, 1866.
C. æstivalis, Curtis 1840;[2] iv, 347, 1847.
C. affinis, R. Br. 1823; xi, 305, 317, 1826.
C. alata, Tor. 1836; xxviii, 231, 1859;[3] Sart. No. 77.
— *straminea*, Sart. Exsic. No. 48 (not Schk.)
C. alopecoidea, Tuckerman Enum. 1843; xli, 326, 1866.
var. sparsi-spicata, Dew., viii, 350, 1849; xliii, 92, 1842.
— *cephalophora* var. *maxima*, Dew., xliii, 92, 1842.
C. alpestris, Allion, vii, 268, 1824; European.
var. tripla, Dew., xxxv, 59, 1863.
C. alpina, Vahl., xxvi, 377, 1834.
C. amplifolia, Boott 1840; iv, 345, 1847.
C. ampullacea, Good. 1792; vii, 266, 1824.
C. angustata, Boott 1840; xli, 326, 1866.
— *acuta*, Muh. (not Lin.), x, 265, 1826; Tor. 1836 and 1843.
C. aperta, Boott 1840; iv, 344, 1847.
— *acuta* var. *erecta*, Dew., x, 265, 1826.
C. aquatilis, Wahl., x, 267, 1826.
C. arctata, Boott 1840, and Illust., No. 124, 1858.
— *sylvatica* (not Huds.), x, 40, 1826.
C. arida, Schw. and Torrey, 1824.
— *Muskingumensis*, Schw. 1823; x, 281, 1826.
C. aristata, R. Br. 1823 (not Dew.); xi, 161, 1826, and xxviii. 240, 1835; xlix, 48, 1845, the Note is wrong.
var. longo-lanceata, Dew., xviii, 102, 1854.
C. atrata, Lin., x, 27, 1826.
var. ovata, Boott 1862; C. ovata, Rudge 1803; x, 44, 1826.
— *C. nigra*, Allion.; also Fries, Anderson and Boott.
C. aurea, Nuttall 1818; x, 48, 1826.
— *pyriformis*, Schw. 1823; ix, 69, 1825.
C. Backii, Boott 1840; xlix, 46, 1845.
C. Baltzellii, Chapman 1845; iii, 355, 1847.
C. Barbarae, Dew., xxxi, 24, 1861: doubtful.
C. Barrattii, Schw. & Torrey 1824; xi, 162, 1826, and xii, 297, 1827; Boott Illust. No. 176, 1858.
C. Bella-villa, Dew., xli, 229, 1866.
C. bicolor, All., xi, 306, 1826.
C. Boottiana, Benthami 1845; xxiv, 47, 1857.
C. bromoides, Schk., viii, 264, 1824.
C. Buckleyi, Dew., xlviii, 143, 1845.
— *Gebhardii*, Buckley (not Schk.).
C. bullata,[4] Schk. 1806; ix, 71, 1825 (not Carey, Boott, &c.).
— var. *cyindracea*, Dew., xi, 315, 1826: cancelled.
C. Buxbaumii, Wabl. 1803; x, 39, 1826; xlii, 245, 1866.
C. Canadensis, Dew., xli, 229, 1866.
C. capillaris, Lin., xxxix, 51, 1840.
C. capitata, Lin., xxxix, 51, 1840.
C. Careyana, Dew., xxx, 60, 1836; xxxv, 59, 1863.
C. Carltonia, Dew., xxviii, 238, 1835, and xli, 326, 1866.
C. cephaloidea, Dew., xli, 326, 1866; Boott, Illust. No. 285, 1858.
C. cephalophora, Muh. 1806; vii, 269, 1824, and x, 268, 1826.
C. Cherokeensis, Schw. 1823; xi, 160, 1826.
C. chordorrhiza, Lin., xlix, 44, 1845.
C. comosa,[5] Boott 1840; ix, 71, 1825.
C. *compacta* (not R. Br.), xxvii, 273, and xxviii, 237, 1835; cancel, lost.
C. compacta, R. Br. 1823.
— *membranacea*, Hooker (not Hoppe), xxix, 247, 1836.

[1] Prof. Tuckerman, in xxxiv, 292, 1862, cancels this name.

[2] Described by Prof. Gray in xlii, 28, 1842.

[3] The caption should read ("Continued from vol. xxvii, p. 81," &c.), and the numbers should be changed from 254, 255 and 256 to 257, 258 and 259.

[4] The description and figure of C. bullata, Schk., are too unlike those of the plant so named by Dr. Boott, 39, 1858.

[5] Substitute this name for that of the species, ix, 71, 1825, that is, for C. pseudocyperus there.

C. concinna, R. Br. 1823; xi, 152, 1826.
C. concolor, R. Br. 1823; xi, 309, 1826.
C. conjuncta, Boott 1858; xxxix, 69, 1865.
— *vulpina*, Sullivant (not Lin.), viii, 348, 1849.
C. conoidea Schk. (not Muh.), x, 47, 1826.
— *granularioides*, Schw. 1823; ix, 262, 1825.
— *Illinoiensis*, Dew., vi, 245, 1848; diseased var.
C. Crawei, Dew., ii, 246, 1846.
var. heterostachia, Dew., ii, 248, 1846.
C. crinita, Lam. 1789; xxvii, 80, 1859; Muh. in part.
var. *paleacea*. *C. paleacea*,[6] Wahl., x, 270, 1826.
C. cristata, Schw. 1823; x, 44, 1826.
C. crus-corvi, Shuttleworth, 1832; Kunze, p. 128, 1850.
— *sicaeformis*, Boott, Boston Journal, 1845.
— *Halei*, Dew., ii, 248, 1846.
— *hystrix*, Gray, and C. *ornithorhyncha*, Fendler.
C. cryptocarpa, Meyer 1830; v, 173, 1848.
— *Scouleri*, Tor. 1836; Boott 1858, No. 158.
C. curta, Gooden. 1792; viii, 93, 1824.
— *Richardi*, Mx. 1803.
— *loliacea*, Dew. (not Lin.), xi, 306, 1826.
C. dascycarpa, Muh. 1817; xi, 148, 1826.
C Davalliana, Smith 1800? xlii, 244, 1866.
C. Davisii, Schw. & Torrey, 1824.
— *aristata*, Dew., vii, 277, 1824 (not R. Br.).
— *Torreyana*, Dew., x, 47, 1826.
C. debilis, Mx. 1803; *C. tenuis*, Rudge 1804.
— *flexuosa*, Schk. 1806; x, 40, 1826.
var. fusiformis, Dew., vi, 244, 1848, and xlii, 244, 1866.
C. decidua, Boott 1846; xxvii, 78, 1859, and xxxi, 26, 1861.
C. decomposita, Muh. 1816; xxv, 140, 1834.
— *paniculata* var. *decomposita*, Dew., x, 275, 1826.
C. Deweyana, Schw. 1823; ix, 62, 1825.
C. digitalis, Willd., xi, 147, 1826.
— *oligocarpa*, Muh. 1817 (not Schk.); x, 280, 1826; Tor. 1836 and 1843.
— *oligocarpa* var. *Van Vlechii*, Dew., x, 281, 1826,
— *Caroliniana*, Buckley[7] 1843; xlviii, 142, 1845.
C. dioica, Lin., x, 283, 1826.
C. disperma, Dew. 1820; viii, 266, 1824.
— *tenella*, Carey, after Carey & Boott (not Schk.[8] or Ehrht.), xix, 256, 1855.
C. disticha, Huds., xli, 330, 1866.
— *intermedia*, Gooden., iv, 343, 1847.
var. Sartwellii, Dew., xliii, 90, 1842, and xli, 330, 1866.
C. Douglasii, Boott 1840; xxiv, 46, 1857.
var. densi-spicata, Dew., xxxii, 41, 1861.
C. eburnea, Boott 1840.
— *alba*, Duv. (not Schk.), vii, 266, 1824, and xi, 316, 1826.
C. Elliottii, Schw. & Tor. 1824; *C. fulva*, Muh. (not good).
— *Baldwinia*, Dew., xxvi, 107, 1834.
C. elongata, Lin., iv, 345, 1847.
C. Emmonsii, Dew., in Torrey, Mon. 1836.
— *Davisii*, Dew., x, 279, 1826.
C. Emoryi, Dew., xxxi, 23, 1861.
C. exilis, Dew., xiv, 351, 1828.
var. squamacea, Dew., same page.
C. extensa, Gooden. 1792; xli, 327, 1866.
C. festiva, Dew., xxix, 246, 1836.
C. festucacea, Schk., viii, 96, 1824; Sart. Nos. 44 and 49, 1848.
— *fœnea*, var. 4, Boott 1862.
— *straminea*, var. festucacea, Boott 1862.
C. filifolia, Nuttall 1818; xi, 150, 1826, and xii, 296, 1827; xli, 329, 1866.
C. filiformis, Gooden. 1792; vii, 268, 1824.
C. flaccosperma, Dew. 1846; corrected, see C. xanthosperma.
C. flava, Lin., ix, 65, 1825.

[6] The mistake of Wahl. and of Schk. is corrected, xxvii, 79, 1859.

[7] Buckley, this Journal, xlv, 173, 1843.

[8] In fact there is no C. tenella, Schk.; for Schk. *himself adopted C. loeliacea* L. *in place* of his name *tenella*.

C. flexilis, Rudge 1803; *C. castanea*, Wahl.
— *blepharophora*, Gray, xxx, 59, 1836.
C. Floridana, Schw. 1823; x, 45, 1826.
C. foenea, Willd., x, 284, 1826, and xxv, 142, 1834.
C. folliculata, Lin. (not Schk. & Muh.).
— *xanthophysa*, Wahl., vii, 274, 1824, and xiv, 353, 1828.
C. formosa, Dew., viii, 98, 124.
C. Franklinii, Boott 1840; xxxii, 38, 1861.
— *ovata*, (not Rudge,) xxvii, 273, 1836.
C. Fraseri, Sims, Bot. Mag.; xi, 155, 1826.
C. fuliginosa, Schk. 1806; xi, 152, 1826.
— *misandra*, R. Br. 1823; xi, 153, 1826; Boott.
C. fulva, Gooden. 1792 (not Muh.)
— *binervis*, Smith, xxx, 61, 1836.
C. fulvicoma, Dew., xxix, 249, 1836.
C. Gayana, Desvous 1847[9] xlii, 244, 1866.
C. Geyeri, Boott 1846; xxvii, 78, 1859.
C. gigantea, Rudge 1803; xi, 164, 1826, and xli, 329, 1866.
C. glabra, Boott 1860; xxxix, 282, 1865.
C. glareosa, Wahl., iv, 344, 1847.
C. glaucescens, Elliott 1824; xi, 150, 1826.
— *verrucosa*, Muh. 1817, var. androgyna,[9] Curtis 1843; xlviii, 140, 1845.
C. gracillima, Schw. 1823; viii, 98, 1824.
C. granularis, Muh. 1817; vii, 272, 1824, and xi, 156, 1826.
C. Grayi, Carey[10] 1847; xxxv, 58, 1863.
C. grisea, Wahl. 1805.
— *laxiflora*, Schk. 1806 (not Lamarck); x, 31, 1836.
C. gynandra, Schw. 1823; xxvii, 79, 1859.
— *crinita* var. *gynandra*, x, 270, 1826; Muh. in part.
C. gynocrates, Wormsk., xxviii, 232, 1859.
— *dioica* var. *Davalliana* (not Wahl.), x, 283, 1826.
C. Halei, Carey (not Dewey), Boott Illust. No. 232, 1860.
— *turgescens* (not Torrey), iii, 356, 1847.
C. Halseyana,[11] Dew., xi, 313, 1826, and xxviii, 231, 1859.
— *polymorpha*, Muh. (not Schk.) in part; Boott 1858.
C. Hartii, Dew., xli, 226, 1866, and var. Bradleyi, 1866.
C. Hartwegii, Boott 1842; xlii, 244, 1866.
C. Haydenii, Dew., xviii, 103, 1854, and xxxv, 60, 1863.
C. Heleonastes, Lin., iv, 345, 1847.
C. hirsuta, Willd., ix, 260, 1825, and xi, 315, 1826.
C. Hitchcockiana, Dew., x, 274, 1826; xxxv, 58, 1863.
C. Hoodii, Boott 1840; xlix, 42, 1845.
C. Hookeriana, Dew., xxix, 248, 1836.
C. Hopneri, Boott 1840; xlix, 46, 1845.
C. Houghtonii, Torrey 1836; xxx, 63, 1836, and xxxix, 73, 1865 (not Sart. 130).
C. hystricina, Willd., x, 35, 1826.
— *Cooleyi*, Dew., a var., xlviii, 144, 1845.
— *Georgiana*, Dew., a var., vi, 245, 1848.
C. ignota, Dew., viii, 348, 1849.
C. incurva, Lightfoot, xxvi, 376, 1834, and xxxii, 39, 1861.
C. intumescens, Rudge 1804; x, 33, 1826.
— *folliculata*, Schk. (not Lin.), x, 32, 1826, and Muh. 1817.
C. Jamesii, Torrey 1836; v, 173, 1848, and xxxv, 60, 1863.
C. juncea, Willd., xli, 327, 1866.
— *miser*, Buckley, xlviii, 141, 1845, and xxix, 346, 1860.
C. Kneiskernii, Dew., ii, 247, 1846.
C. lævi-conica, Dew., xxiv, 47, 1857, and xxxv, 60, 1863.

[9] Description by Dr. Curtis in this Journal, xliv, 84, 1843.

[10] Mr. Carey's description in iv, 22, 1847.

[11] C. Halseyana is one of the three distinct species, imperfectly described, under C. polymorpha, Muh. (not Schk.); C. striata, Mx., is another of them; and the third is not yet ascertained. No one of them can, with propriety, be named C. polymorpha, Muh., because the name does not designate any one. This is acted out by Dr. Boott in Illust., No. 56, 1858, as named by him, "C. polymorpha (Muhlenberg) (C. Halseyana, Dewey)." Dr. Boott evidently saw that C. polymorpha, Muh., did not designate the species, and hence added C. Halseyana, Dew., which did show the species intended. C. polymorpha, Muh., *in part*, is even too bad when avoidable.

C. lævigata, Smith, Brit. Flora.
— *Greeniana*, Dew., xxx, 61, 1836.
C. lagopina, Wahl., v, 172, 1848.
C. lagopodioides, Schk., viii, 95, 1824.
C. lanceata, Dew., xxix, 249, 1836.
C. languinosa, Mx. 1803.
— *pellita*, Muh. 1806; ix, 70, 1825.
C. laxiflora, Lam. 1789 (not Schk.); xxvii, 80, 1859.
— *anceps*, Muh., x, 36, 1826.
var. patulifolia, Dew., and var. angustifolia, Dew., 1845.
C. Leavenworthii, Dew., ii, 246, 1846.
C. leiocarpa, Meyer 1830; v, 174, 1848.
C. lenticularis, Mx. 1803 (not Dewey); v, 175, 1848; xxviii, 232, 1859, and xxix, 348, 1860.
C. lepidocarpa, Tausch, iii, 172, 1847; var. of C. flava, L.
C. leporina, Lin., xxvii, 272, 1835.
— *ovalis*, Good. 1792; vii, 276, 1824, and xxvi, 277, 1834.
C. leucoglochin, Ehrht., x, 42, 1826.
C. Liddoni, Boott 1840; xlix, 45, 1845.
C. limosa, Lin., x, 41, 1826.
— *lenticularis*, Dew. (not Mx.), vii, 273, 1824.
— *laxa*, (not Wahl.) xxvi, 376, 1834.
var. radicalis, Paine, xxxix, 71, 1865.
C. livida, Willd., and var. radicalis, Paine, xli, 329, 1866.
— *limosa* var. *livida*, Wahl. 1803; x, 41, 1826, and xli, 329, 1866.
— *Grayana*, Dew., xxv, 141, 1834.
C. longirostris, Tor., ix, 257, 1825.
C. lupulina, Muh. 1806; xi, 165, 1826.
var. 2, gigantoidea, Dew. 1865; xli, 328, 1866.
C. lupuliformis, Sartwell, ix, 29, 1850.
— lupulina var. polystachya, Schw. & Torrey 1824; xi, 166, 1826.
C. lucorum, Willd., xix, 252, 1855. Doubtful.
C. Lyoni, Boott 1840; xlix, 42, 1845.
C. Macounii, Dew., xli, 228, 1866.
C. macrocephala, Willd., xliii, 91, 1842.
C. macro-chaeta, Meyer 1830.
— *spectabilis*, Dew., xxix, 248, 1836.
C. Magellanica, Lam. 1789; xxxix, 70, 1865, and Boott 1860.
— *limosa* var. *irrigua*, Wahl. 1803; x, 41, 1826.
— *paupercula*, Mx. 1803 and Torrey 1843; xlii, 330, 1866.
— *irrigua*, Smith 1845; x, 42, 1826.
C. marcida, Boott 1840; xlix, 43, 1845.
C. marina, Dew., xxix, 247, 1836.
C. maritima, Vahl., iv, 343, 1847.
C. media, R. Br. 1823; xi, 309, 1826.
C. Meadii, Dew., xliii, 90, 1842; Boott 1858.
C. Mertensii, Prescott 1833?
— *Columbiana*,[12] Dew., xxx, 62, 1836.
C. microdonta, Torrey & Hooker 1836; v, 174, 1848, and xxxv, 60, 1863.
C. microglochin, Wahl., v, 174, 1848.
C. microstachya, Mx. 1803 (not Erhrt, earlier), Mon. 1836.
— *polytrichoides*, Muh. 1806; ix, 258, 1825.
C. miliacea, Muh., x, 30, 1826.
C. miliaris, Mx. 1803; x, 36, 1826, and xli, 330, 1866.
C. mirabilis, Dew., xxx, 63, 1836.
C. mirata, Dew., xxxix, 71, 1865.
var. minor, Dew., same date; doubtful.
C. Mitchelliana, Curtis,[13] xlviii, 140, 1845.
C. monile, Tuckerman, xxix, 346, 1860; Exsic. 152, 1848, probably true.
C. Muhlenbergii, Schk., viii, 265, 1824.
C. muricata, Lin., xi, 307, 1826.
C. mutica, R. Br. 1823; xi, 310, 1826, and xxix, 252, 1836.
C. nardina, Fries 1838? iv, 246, 1847.
— *Hepburnii*, Boott 1840.
C. Nebraskensis, Dew., xviii, 102, 1854, and xxxv, 60, 1863.
C. nigricans, Meyer 1830; xxix, 249, 1836.

[12] Named before Prescott's work had come to hand, as in some other cases.
[13] See his description, this Journal, xliv, 84, 1843.

C. nigro-marginata, Schw. 1823: x, 282, 1826.
C. Novæ-Angliæ, Schw. 1823; ix, 64, 1825.
— *collecta*, Dew., a var., xi, 314, 1826.
C. Norvegica, Wahl., xxxii, 38, 1861.
C. Nuttallii, Dew., xliii, 92, 1842.
— *Meekii*, Dew., a var.; xxiv, 48, 1857.
C. obtusata, Lilj.
— *Backana*, Dew., xxix, 259, 1836.
C. Oederi, Ehrht. 1800; x, 38, 1826, and xlii, 245, 1866.
— *viridula*, Mx. 1803; xi, 153, 1826, and xlii, 245, 1866.
C. oligocarpa, Schk. (not Muh.) 1806; iv, 349, 1847.[14]
var. Sartwelliana, Dew., v, 176, 1848.
C. oligosperma. Mx. 1803; xi, 160, 1826.
— *Oakesiana*, Dew., xiv, 351, 1828.
C. Olneyi, Boott 1858; xxix, 347, 1860.
C. oxylepis, Tor. & Hook. 1836; iii, 354, 1847.
C. pallescens, Lin., vii, 267, 1824.
C. paludosa, Good. 1792; xxix, 346, 1860.
C. panicea, Lin., xxv, 140, 1834.
C. paniculata, Lin., x, 275, 1826, for its varieties.
C. paradoxa, Schk., iv, 346, 1847.
C. Parryana, Dew., xxviii, 239, 1835, and xlii, 331, 1866.
— *arctica*, Dew., the same, *young*.
C. pedunculata, Muh. 1806; ix, 259, 1825.
C. Pennsylvanica, Lam. 1789; xix, 252, 1855.
— marginata, Muh. 1806, xi, 163, 1826; var. Dew., xxxv, 59, 1863.
C. petasata, Dew., xxix, 246, 1836.
C. petricosa, Dew., xxix, 246, 1836.
C. physema, Dew., xxix, 347, 1860.
— *bullata*, Carey 1848, or Boott 1858 (not Schk.).
C. plantaginea, Lam. 1789, Muh. & Schk. in part; vii, 272, 1824, and xi, 155, 1826.
C. platyphylla, Carey,[15] viii, 349, 1849.
C. podocarpa, R. Br. 1823; xi, 162, 1826, and xxix, 251, 1836.
C. præcox, Jacq.
— *verna*, Vill., xi, 314, 1826.
C. pseudo-cyperus, Lin., iv, 348, 1847.
C. pubescens, Muh., ix, 73, 1825, and xli, 330, 1866.
C. pyrenaica, Wahl., iv, 346, 1847.
C. Raeana, Boott 1851 and 1858; xli, 230, 1866.
C. rariflora, Smith 1845; xxxix, 71, 1865.
— *limosa* var. *rariflora*, Wahl. 1803; x, 42, 1826.
C. Raynoldsii, Dew., xxxii, 39, 1861.
C. recta, Boott 1840; v, 175, 1848.
C. Redowskiana, Meyer 1830; xxix, 250, 1836.
C. remota, Lin., xi, 309, 1826.
C. retrocurva, Dew. 1845; xlii, 243, 1866.
— oligocarpa, Muh. (not Schk.), var. latifolia, Gray 1835.
C. retroflexa, Muh. 1806; vii, 271, 1824, and x, 277, 1826.
C. retrorsa, Schw. 1823; ix, 67, 1825.
C. Richardsoni, R. Br. 1823; xi, 152, 1826.
C. rigida, Good. (not Carey), xlix, 45, 1845.
— *saxatilis*, Fl. Dan. Oeder 1764; xi, 310, 1826 (not Lin.).
C. riparia, Curtis 1792; xlix, 47, 1845, and xxxv, 60, 1863.
— *lacustris*, Willd., x, 43, 1826; united, Tor. 1836.
var. laxiflora, Dew., xxxv, 60, 1863.
C. rosea, Schk., x, 276, 1826.
var. radiata, Dew., same reference.
C. Rossii, Boott 1840; xxxv, 57, 1863.
C. rostrata, Mx. 1803 (not Schk. or Muh.); xxxix, 52, 1840.
— *xanthophysa*, Wahl., var. *nana* and *minor*, Dew., xiv, 353, 1828; xxxix, 73, note, 1865.
C. rotundata, Wahl. 1803; xli, 327, 1866.
C. rupestris, Allion.
— *attenuata*, R. Br. 1823; xi, 305, 1826.
— *Drummondiana*, Dew., xxix, 251, 1836.
C. salina, Wahl. 1803; iv, 344, 1847.
C. saxatilis, Lin. 1737; xxviii, 236, 1835.

[14] This description overlooked by Dr. Boott in his No. 93, 1858.
[15] Description by Mr. Carey, in iv, 23, 1847.

— *pulla*, Gooden. 1792; Boott 1843; Fries 1846.
C. scabrata, Schw. 1823; ix, 66, 1825.
C. scabrior, Sartwell, viii, 349, 1849.
C. Schkuhrii, Willd., xxviii, 238, 1835.
— *supina*, Willd., xxvi, 376, 1834.
C. Schottii, Dew., xxxi, 25, 1861.
B. Schweinitzii, Dew., ix, 68, 1825.
C. scirpoidea, Mx. 1803; xi, 154, 1826.
— *Wormskioldiana*, Hornm, xiv, 352, 1828; xi, 154, 1826.
C. scirpoides, Schk., viii, 96, 1824.
C. scoparia, Schk., viii, 94, 1824.
C. setacea, Dew., ix, 61, 1825.
C. Shortiana, Dew., xxx, 60, 1836; C. Schortii, Tor., later.
C. siccata, Dew., x, 278, 1826.
— *pallida*, Meyer 1830.
C. Sitchensis, Prescott 1827?
— *cryptocarpa*, (not Meyer,) xxix, 245, 1836.
C. sparganioides, Muh., viii, 255, 1824.
var. minor, Boott 1862.
— *muricata* var. *cephaloidea*, Dew., xli, 330, 1866.
C. squarrosa, Lin. 1757; vii, 270, 1824.
var. typinoides, Dew., xi, 316, 1826.
C. stellulata, Gooden. 1792; xi, 306, 1826.
C. stenolepis, Tor. 1835; xxx, 59, 1836, and xli, 331, 1866.
C. stenophylla, Wahl. 1803; xxviii, 237, 1835.
C. sterilis, Willd., xi, 304, 1826.
C. steudelii, Kunth 1835; xlix, 46, 1845.
C. stipata, Muh., vii, 271, 1824.
C. straminea, Willd., vii, 276, 1824, and xi, 157, 1826.
var. brevior, Dew., xi, 158 and 318, 1826.
— var. aperta, Boott 1862, and C. tenera, Olney.
C. striata, Mx. 1803; xxviii, 231, 1859.
— *polymorpha*, Muh., iii, 255, 1847, and xlii, 329, 1866, note.
— *Houghtonii*, Sart. No. 130 (not Torrey).
C. striatula, Mx. 1803; xxvii, 276, 1835, and xlii, 245, 1866.
— *anceps*, in part Am. authors; Mon. 1836.
— *blanda*, Dew., x, 45, 1826; Torrey 1843.
— *conoidea*, Muh. (not Schk.) 1817.
C. stricta, Gooden. 1792 (not Lam.); x, 269, 1826.
C. styloflexa, Buckley[16] 1843; xlviii, 141, 1845.
C. stylosa, Meyer 1830; xlii, 243, 1866, and xxix, 252, 1836.
C. subulata, Mx. 1803 (not Wahl.).
— *Collinsii*, Nuttall 1818; xi, 317, 1826.
— *Michauxii*, Dew., x, 273, 1826; as subulata, used by Wahl.
C. Sullivantii, Boott[17] 1840; xlix, 44, 1845.
C. sychnocephala, Carey.[18]
— cyperoides (not Lin.), iii, 171, 1847.
C. tenax, Chapman, xix, 254, 1855; Sart., 113, 1848.
C. tenella, Ehrhart 1800? (not Schk. or Carey); xxviii, 232, 1859.
— *canescens* var. *alpicola*, Wahl.
var. *sphærostachya*, Tuck. En. 1843.
— *sphærostachya*, Dew., xlix, 44, 1845.
— *vitilis*, Fries 1846; Anderson 1849.
— *canescens* var. *vitilis*, Carey 1848.
— *Persoonii*, Sieb., xix, 253, 1855.
C. tenera, Dewey, viii, 97, 1824.
C. tentaculata, Muh., x, 34, 1826.
var. gracilis, Boott, 231, 1860.
var. rostrata, Sartwell (not Schk. or Mx.), Exsic. 1848.
var. parvula, Paine, Cat., 157, 1865.
C. tenuiflora, Wahl., xxxix, 51, 1840.
C. teretinscula, Gooden. 1792; vii, 265, 1824.
C. tetanica, Schk. (not Muh.), xi, 312, 1826.
C. Thurberi, Dew., xxxi, 24, 1861, and xxxv, 60, 1863.
C. Torreyi, Tuck. 1843; xlix, 43, 1845.

[16] Description by Mr. Buckley in this Journal, xlv, 174, 1843.
[17] Description by Dr. Boott in xlii, 39, 1842.
[18] Mr. Carey's description in iv, 24, 1847.

C. torta, Boott 1843; ix, 30, 1850.
— *cæspitosa*, (not of Lin. or Gooden.), x, 266, 1826.
var. ramosa, Dew., xii, 297, 1827.
— *acuta*, in part (not Lin.), x, 265, 1826, and var. sparsiflora, Dew.
C. triceps, Mx. 1803; xlviii, 142, 1845.
C. trichocarpa, Muh. 1806; vii, 274, 1824, and xi, 158, 1826.
var. turbinata, Dew., xi, 159, 1826.
C. triquetra, Boott 1845; xxxv, 60, 1863.
— *monticola*, Dew., xxxi, 26, 1861.
C. trisperma, Dew., ix, 63, 1825.
C. Tuckermani, Dew., xlix, 48, 1845, and Boott 1846.
C. turgescens,[18] Tor. 1836; xxxv, 57, 1863.
C. umbellata,[19] Schk. 1806; x, 31, 1826, not xxxi, 26, 1861.
var. vicina, Dew., xi, 317, 1826.
C. ursina, Dew., xxviii, 240, 1835.
C. ustulata, Wahl., xi, 149, 1826, and Schk.
C. utriculata, Boott 1840; xxviii, 231, 1859.
var. sparsiflora, Dew., xxviii, 232, 1859.
— *cylindrica*, Schw. 1823 (not Tuck. or Carey); xli, 331, 1866.
C. vaginata, Tousch 1821; xli, 227, 1866; Kunze 1850.
var. alti-caulis, Dew., xli, 227, xlii, 245, 1866.
C. Vahlii, Schk. 1806; xxvi, 377, 1834.
C. vallicola, Dew., xxxii, 40, 1861.
C. varia, Muh.[20] 1806; xi, 162, 1826.
var. pedicellata, Dew., xi, 163, 1826.
C. Vaseyi,[21] Dew., xxix, 347, 1860, and xli, 331, 1866.
— *monile*, Sartwell (not Tuckerman), xlix, 47, 1845.
— *vesicaria* var. *cylindracea*, Dew. 1845.
C. venusta, Dew., xxvi, 107, 1834.
C. verrucosa, Schw. (not Muh.), xi, 159, 1826; mistake of Schw., and is expunged.
C. verticillata, Boott 1858; xli, 230, 1866.
C. vesicaria, Lin., x, 273, 1826; Boott 1846.
C. vestita, Willd., ix, 261, 1825.
C. virescens, Muh., ix, 259, 1825.
C. vulpinoidea, Mx. 1803.
— *multiflora*, Muh. 1806; ix, 60, 1825.
var. microsperma, Dew., xi, 317, 1826.
C. vulgaris, Fries 1846, Nov. Mant.
— *cæspitosa*,[22] Gooden. 1792, & Muh. (not Lin.), xii, 297, 1827, last of the note.
C. Washingtoniana, x, 272, 1826, and xii, 296, 1827.
— *rigida*, Carey (not Gooden.) in Gray, 1848.
C. Wildenovii, Schk., ix, 258, 1825, and xi, 311, 1826.

[18] C. turgescens, iii, 356, 1847, should be named C. Halei, Carey (not Dewey).

[19] In the description of this species I added to that of Schk. and Muh. the characters of the form *vicina*, which they had never seen, and thus I introduced under their name *umbellata*, a plant with other characters which their language did not admit, and yet I called it by their name. In the next volume I separated this *vicina*, characterizing it as *var. vicina* of C. umbellata, Schk. I thought the truth required this, that I might give to Schk. and Muh. their right.

[20] On the principle of the last note, when I found plants closely resembling C. varia, Schk. & Muh., except not having *their sessile* spikelets, I named them C. varia, Muh., var. pedicellata, as these were not found by Muh. till long after Schk. had published C. varia. By blending the characters, the *var.* becomes the *type*, and the *type* of Muh. becomes the *variety*, as in Boott, Illust., No. 239, 1860.

[21] Recent examinations show that C. Vaseyi is probably only an immature form of C. monile, Tuck., as Dr. Sartwell named it in his No. 152, Exsic.; yet both Dr. Boott and Prof. Tuckerman rejected it. As the locality has been destroyed, Dr. S. cannot obtain mature specimens there for proof.

[22] C. cæspitosa, Gooden., an English Carex, was confounded with, or itself absorbed, C. cæspitosa, Lin., from 1792 till they were shown to be distinct and were separated by Dredger, as Dr. Boott states, in 1841. As the former is found in New England, and widely over the north and west, it is placed under the name given it by Fries, as above, C. vulgaris, to prevent mistakes for the Linnæan name.

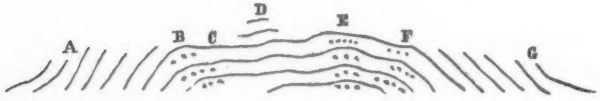
C. Woodii, Dew., ii, 249, 1846, and iv, 349, 1847.
— *titanica*, Muh. (not Schk.), iv, 349, 1847.
C. Wrightii, Dew., xxxi, 24, 1861, and xxxv, 60, 1863.
C. xanthosperma,[33] Dew., ii, 245, 1846.
— *flacco-sperma*, Dew. 1846, and Boott 1858.
C. xerocarpa,[34] S. H. Wright, xlii, 334, 1866, note.

Art. XLV.—*On the Oil-producing Uplift of West Virginia;* by Prof. E. W. Evans.

The most productive oil region of West Virginia, so far as developments have yet gone, is confined to a line of uplift, extending from Burning Springs on the Little Kanawha, in a direction from twelve to fourteen degrees east of north, to the Ohio river,—a distance of 35 miles. Thus limited, the line is nearly bisected by the Northwestern Virginia railroad, at Petroleum station; which will therefore serve as a convenient point of reference.

In its central portion, which for convenience I shall designate as the middle segment, the upheaval is most marked, and exhibits some distinctive features. This part is about ten miles in length, and is also nearly bisected by the railroad. A cross-section of it, as made out from the railroad cuttings, the records of borings, and the frequent exposures of rock along the streams of the neighborhood, is shown in fig. 1. It is a decapitated

1.



fold,—a flexure of the strata in the anticlinal form, having the summit worn down by water.

[33] This name should be substituted for *flaccosperma* in the volume and page here given, as the name was selected on account of the, *yellow* color of the fruit on the plants received from the south: *yellow fruited* Sedge. C. flaccosperma is an accidental miscopy.

[34] 304. *C. xerocarpa*, S. H. Wright. Spikes 4–6, long, very slender erect; the staminate 2–4, sessile or sub-pedicelled, with close ovate acute scales; the pistillate 1–2, close fruited except the lower very lax part, the upper half staminate; stigmas 2; fruit ovate short-acute, small and close, compressed and apiculate, longer than ovate acute scale, or equalling it, except some of the upper and long lanceolate scales; culm 18 to 24 inches, erect, slender, 3-sided and scabrous on the upper part; bracts leaf-like; leaves slender, narrow, scabrous on the edge and shorter than the culm; plant, except the dry brownish *arid* spikes and fruit which give the name, dark green.

Prattsburg, Steuben Co., N. Y., on a rich peat bottom in an extensive flat, abundant over one-fourth of an acre, without any other Carex.—*Dr. S. H. Wright.* Of the *stricta* species, this is the *strictest*, with the longest small spikes and close smallest fruit.

There is a steep western slope, from A to B, where the rocks are tilted up at an angle varying from 45° toward B to upwards of 60° toward A. The distance across these upturned edges, from A to B, measured horizontally is about 1200 feet. After allowing for the divergent dips, I estimate the original thickness of rock here represented to be about 850 feet.

In the eastern steep slope, from F to G, the strata incline at an angle of from 30° to 40°; but the distance across, taken horizontally, is so much greater as to render the thickness of rock thus displaced about the same as in the western slope.

To the westward from A, and to the eastward from B, there is a somewhat sudden transition from the steep slopes to a comparatively slight inclination of the strata, which continues far enough to either side to give an additional outcropping of about 300 feet, estimated perpendicularly to the strata.

It is not claimed that these figures are accurate; but if they are approximately correct, the rocks on the summit of the anticlinal have been brought up out of place not less than 1100 feet, probably as much as 1200 at the highest axial point. This estimate has reference to the place of maximum upheaval, a little north of the railroad.

In the inner belt between the steep slopes, which is about a mile in breadth, from B to F, the strata are nearly horizontal, but somewhat wavy—the waves partaking of the direction of the main uplift. One of these, represented at E, is quite prominent and persistent, and in fact constitutes the crown or proper axial line of the anticlinal. It is much nearer the eastern slope than the western.

The most distinctive features of this segment of the uplift are seen in the abrupt change of direction at B and F. The Appalachian folds farther east, where there was more heating and crystallization, exhibit curves rather than angles, even when most prominent. But here we find sharp angular points, with some arching to either side, but upon the whole partaking rather of the character of fractures, and affording numerous crevices in the soft and brittle rock. Hence the popular name of "the break," by which this uplift is known, is not altogether inapplicable. There is, however, no actual disruption of the strata, and no fault, either at the western angle B, or at the eastern angle F. The abruptness of these angles has led some into the

2.

supposition that the nearly horizontal strata of the inner belt are not continuous and identical with the inclined strata to

either side, but abut against their sides or faces, in the manner exhibited in fig. 2. That such is not the case is conclusively shown by the following facts.

First, a comparison of the rocks in the western slope, from B to A, fig. 1, by their lithological characters, with the rocks in the eastern slope, from F to G, shows that they are identical one by one in their order; they are also found identical with the rocks in the intermediate hills, as at D, so far as these extend upward, say a maximum height of about 300 feet. The strata thus traced over include sandstones, shales and veins of coal; also a layer of flinty limestone, which may be mentioned as one of the best guides. At the railroad this appears on the west side projecting upward through the bed of Laurel Fork, and on the east side in an exposure of sloping rocks in the hill a little south of Petroleum station. At various places it not only appears in the opposite slopes, but has also been traced through the hills between, as on Comb run, about two miles south of the railroad. Indeed there are numerous exposures in situations corresponding to B and F, fig. 1, where the continuous connection of the same strata, as they change from an approximately horizontal position to a steep pitch, is plainly traced by the eye.

Again, it is a fact that the rocks bored through in positions a little to the left of B, fig. 1, correspond, in lithological characters, with those found to the right of it, but differ from the latter in being struck further down, and in continuing longer in proportion to a greatly increased inclination. Borings made at B descend into rocks of steep pitch. All this is in direct contradiction to the hypothesis presented in fig. 2, where the sloping rocks are represented as not only distinct from, but also as projecting over the horizontal ones.

The inner belt between the slopes, from B to F, is not inappropriately called the "oil belt," as it is only in the horizontal or slightly inclined rocks between these limits that oil has been found in paying quantities. In the upturned edges of the east and west slopes, as well as over a considerable extent of territory outside, a large number of wells have been bored for oil, but with little or no success.

Even within the inner belt, the producing wells are thus far nearly all confined to three narrow strips. It will be seen that the cross section, fig. 1, exhibits three principal angles, marking three distinct axial lines; the western angle at B, which is the most prominent, next the eastern angle at F, then the inner angle at E, which is the least prominent. It is along these three axes that all the good wells are located.

The most successful developments are along the western margin of the inner belt, on a strip about 500 feet in width, from B to C, fig. 1. On the eastern branches of Laurel fork, on White

Oak fork, and other small streams from one to four miles north of the railroad, and on the intervening hills, a great many wells have been sunk on this line; and a majority of them are yielding oil, in some cases from 100 to 300 barrels per day, but generally from 10 to 100.

The oil is usually found in a brittle light-gray or white sandstone, or group of sandstones, from 150 to 350 feet below the level of the streams; the depth varying somewhat, however, with different localities. Crevices are sometimes struck at a less depth, especially on the side toward B; but they are generally dry crevices, or contain only water. As the principal line of fracturing (given the fragility of the rock) would follow the axis plane, approximately bisecting the angle at B, it would of course incline to the eastward from B in its descent, as represented in fig. 1. Accordingly the most frequent strikes, and generally the best wells, are not on the side next to the slope, but on the eastern half of the strip, toward C, where this main line of crevices, in its eastern ramifications, would cross the above-mentioned sandstone, after it has become horizontal.

On the eastern margin of the inner belt, the main if not the only developments are on Gales' fork of Myers' creek, about three and a half miles north of the railroad, where there are some good wells, confined to a similar narrow strip, just west of F; but the production is not so large, in proportion to the number of wells sunk, as on the western margin.

On the inner axis, at E, and a short distance to either side of it, there have been numerous strikes on Oil Spring run, from one to two miles north of the railroad, and on Hughes river near the California House, about four miles south of the railroad; but the yield of oil at these localities is small. .

Thus it is seen that the quantity of oil found bears something like a direct ratio to the amount of fracturing. Many borings have been made in the areas that lie between the three productive strips, and a few of them, located on minor flexures, have been partially successful. Deep boring will probably reduce the barren strips to narrower limits, but the one or two deep wells that have been already sunk, between the western and inner angles, are too far off to approach the region of the axis-plane of either.

This large accumulation of oil in the crevices of the anticlinal would seem to be owing, not solely to a direct connection by a continued line of fractures with the original sources of the oil in strata beneath, but in part also to the collection, from a wider area, of oil that has come up elsewhere, as through the crevices of the adjacent synclinals. For, being lighter than water, it would gradually work up between the strata of the slopes. Where these are decapitated it has escaped to the sur-

face; but the inner (or lower) strata of the slopes conduct to crevices still covered by a mass of horizontal rock, where oil is now found.

The surface rocks of the country adjacent to this uplift, on either side, are well known to be those of the Upper Coal-measures. The lowest rocks brought to the surface by the upheaval are undoubtedly those of the Lower Coal-measures. This is indicated by the occasional appearance of Lepidodendra. In the hills between the slopes there are three or four veins of coal, and their equivalents are found in the inner strata of the slopes; but where the upheaval has been greatest, I think only two seams have been detected in wells sunk in the valleys;—those are very thin, and both occur over 100 feet below the surface. From 150 to 500 feet down (in approximate figures) the rocks are mainly white sandstones with some conglomerate and salt water. This, or the main part of it, is the conglomerate or millstone grit series, which in West Virginia is mainly a sandstone, and constitutes the principal salt rock. From 500 to 850 feet down there is found a series of light blue shales, interstratified, toward the bottom, with some thin layers of conglomeratic sandstone. This, or the main part of it, is probably the upper group of Subcarboniferous rocks (the Umbral of Rogers), which in Pennsylvania and the northern parts of West Virginia is composed chiefly of shales, but to the south and west becomes calcareous. Below 850 feet is a sandstone which has not yet been penetrated a great distance. This may be the Lower Subcarboniferous (or Vespertine); or, as from data furnished by Rogers we may infer that the latter is but slightly developed in this region, and may be represented by the thin conglomerates just mentioned, it is probable that the sandstone belongs to the Chemung group, overlying the black shales, and constituting the geological horizon of most of the oil obtained in Pennsylvania.

The opinion has been expressed, in this Journal, that a well 860 feet deep, put down near the middle of the inner belt, in the neighborhood which we are considering, and at a point where it is represented that a depth of about 1000 feet from the top of the Upper Coal-measures down has been eroded, passes down through the lowest Carboniferous rocks, then through the Waverly sandstone (or supposed equivalent of the Chemung group), then through 265 feet of the equivalents of the Black shales, far down in the Devonian.

To this hypothesis there are strong objections. It makes the strata included between the top of the Upper Coal-measures and the Black shales much thinner in the aggregate at this point than they are estimated, in the Ohio reports, to be in the neighboring counties to the northwest of the Ohio river. This is in

direct contradiction to the general fact, as set forth by Rogers and others, that there is a thickening of these strata on going southeastward toward the mountains. Especially does it seem necessary to allow an appreciable thickness at this point to the upper or shaly group of the Subcarboniferous, which, according to Rogers, has a much more extensive distribution westward, from its eastern outcrop in the Appalachian chain, than the lower or sandstone group. I am aware that at the northwestern outcrops in Ohio it is scarcely, if at all, represented. But its increase to the southeast is so great, attaining as it does a thickness of 3000 feet at the mountains, that it is not difficult to suppose it has a thickness of 300 feet at this point,—especially as at the Great Kanawha salines eight miles above Charleston, a point not very much nearer its line of maximum development than this, it is found in boring to have a thickness of not less than 700 feet. Besides, the 300 feet of shales which I have set down as the Upper Subcarboniferous bear very little resemblance to the highly bituminous group known as the Black shales. They are not black, neither are they bituminous. The occasional occurrence of streaks of dry carbonaceous matter which, as it comes up with the fine borings and floats on the water, resembles powdered charcoal, and is known as "soot," does not distinguish these shales from the Carboniferous strata overlying them. The theory suggested, that they have been decolorized by heating and debituminization, hardly comports with the fact that among the eastward Appalachian ridges, where the heating has been so much greater than here that the coal is debituminized, and the black shales themselves not only debituminized but metamorphosed into slates, they still retain their characteristic dark coloring of carbonaceous matter. If anything more were wanting to show that the group above referred to is not the equivalent of the Black shales, it would seem to be supplied by the fact that the rock underlying them is found to be sandstone, and not limestone, as that hypothesis would require.

Thus far I have confined my remarks chiefly to the middle segment of the uplift, with which I am personally best acquainted. I am indebted for many important facts, especially in regard to the northern and southern segments, to Col. A. J. Warner, a practical geologist who has repeatedly traversed the country the whole length of this line, for the purpose of exploration.

As we follow it northward toward the Ohio river, and southward toward the Little Kanawha, the upheaval becomes less marked, and assumes a somewhat different character. The eastern and western axes become less and less prominent, and the middle axis becomes more prominent by comparison, though really subsiding; and this finally becomes the sole axis, toward

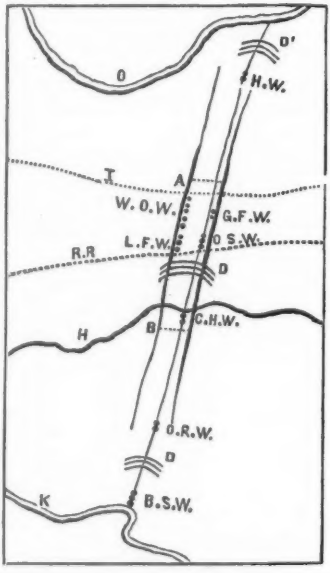
both extremities of the line. At the northern and southern ends of what I have designated as the middle segment, a little north of the Northwestern Virginia turnpike, and a little south of Hughes river, the subsidence of the eastern and western axes becomes somewhat sudden and marked. Both however can be clearly traced a few miles further, northward across the headwaters of Bull creek to within five or six miles of the Ohio river, and southward across the headwaters of Standing Stone creek to within seven miles of Burning Springs. The western axis seems to extend a little further, both ways, than the eastern.

It has been asserted that the middle portion of this uplift so narrows down toward both ends that the opposite slopes actually come together, thus enclosing an insular space, in the form of a double convex lens. A careful examination will show that such is not the fact; so far from it that toward their extremities the eastern and western axes actually diverge, as one after another of the inner strata of the slopes sink to a lower inclination.

Both at Burning Springs and on the Ohio, the cross section exhibits only a simple anticlinal or flexure, whose axis is the prolongation of what has been called the middle axis. Strictly speaking, these are not the extreme points of the uplift; it has been traced in a diminishing wave some miles across both rivers; and probably its whole length is as much as 50 miles. It is replaced, toward both ends, by other and minor flexures, approximately parallel with it.

At nearly all points along this uplift where a thorough test has been made, on the lines before indicated, oil has been found in good quantity. The principal developments on the southern segment are at Burning Springs on the Little Kanawha, and at

3.



B. S. W.—Burning Spring Wells.
C. H. W.—California House Wells.
D—Dips.
G. F. W.—Gales Fork Wells.
H—Hughes river.
H. W.—Horseneck Wells.
K—Little Kanawha river.
L. F. W.—Laurel Fork Wells.
O—Ohio river.
O. R. W.—Oil Rock Wells.
O. S. W.—Oil Spring Run Wells.
R. R.—Northwest Virginia Railroad.
T—Northwest Virginia Turnpike.

Oil Rock on the headwaters of Standing Stone creek. These are on or near the main axial line. Some deep wells recently sunk yield as much as 500 barrels per day. On the northern segment there are some producing wells in a similar position on Horseneck, about three miles from the Ohio river.

The annexed cut, fig. 3, showing approximately the course and relative positions of the three axes, and the locations of producing wells, will, for the most part, explain itself. A and B are the extremities of the middle segment. To avoid crowding, the smaller streams have been omitted, and the intervals between the axes enlarged.

The oil found in the middle segment is mostly heavy or lubricating oil; that found in the northern and southern segments is mostly light oil. A probable explanation of this is afforded by the theory that the heavy and less volatile oil is a residuum, remaining after the evaporation and drainage of the lighter oils, through crevices connecting with the surface. The angles being more abrupt in the middle segment, and affording more open crevices, the escape of the lighter oil would be more rapid. Another fact of similar significance is, that even here light oil is often found along the inner or least abrupt of the three angles. This is the case at the California House, and, to a less extent, on Oil Spring run.

The wells of the middle segment obtain their supplies mainly from the upper part of that group of sandstones which we have considered as representing the conglomerate series. The deep wells at Burning Springs obtain light oil from the same geological horizon; but the shallow wells of the northern and southern segments derive their oil from the rocks of the Coal-measures. A deep well near the railroad, in the middle segment, which has penetrated over 100 feet into the probable equivalents of the main oil rocks of Pennsylvania, yields, from the bottom, an immense quantity of carburetted hydrogen but no petroleum.

On this as well as other anticlinals in this region, burning springs are of quite common occurrence, but they are in most cases locally associated with light oil. Many of the wells from which light oil is obtained afford gas enough to throw the oil and water above the surface. In some cases this takes place on the fire-engine principle, the compressed gas being confined and the liquids alone escaping; in other cases the escaping gas forces out before it whatever liquids it meets in its way.

The tendency to break into square or rhombic blocks, so as to afford an extensive system of vertical fissures, would seem to be one of the determining characters of the main oil rocks. This feature belongs to several of the coarse sandstones of the Coal-measures; but it is most conspicuously seen in the conglomerate

at its various outcrops, as in Hocking county, Ohio, and at the falls of the Great Kanawha;—so also near the head of the Alleghany river, where the groups of regular blocks, standing out upon the surface, have given rise to such names as Rock City and Ruined City. In some cases, however, the oil-yielding rock is found crushed into small fragments.

This line of uplift is approximately parallel with the Appalachian folds to the eastward, and is undoubtedly a member of the same system. The statement has appeared in this Journal that it makes an angle with the mountain chains of about 40°, —an error arising probably from comparing it with the mountains of Tennessee or northern Pennsylvania, instead of with those which lie in a lateral direction from it, namely, in northern Virginia. The latter constitute one of those segments of the Appalachian zone where the mountains, according to Rogers, bear approximately north and south. This flexure is as nearly parallel with their general course as they are with one another. Nor does it stand isolated from the rest; for in the intermediate space there are several other minor flexures, having the same general direction, though occurring at intervals which increase to the westward.

As establishing the theory that the lateral pressure concerned in uplifting the Appalachian folds was exerted from the ocean side (as if by the subsidence of a submarine area in a period of great cooling and contraction), it has been shown by geologists that as a general fact the western dips of the folds are steeper than the eastern,—that upon going westward, further from the direct action of the moving force, this feature becomes less marked,—that to the westward also the folds are less crowded together, or separated by wider intervals, and the uplifting connected with less metamorphism and debituminization. This fold, as one of the westernmost members of the series, conforms to the rest in all these general laws which, with the fact of parallelism, serve to connect them with a common cause, as parts of one system.

Toward the extremities of this uplift, where it becomes a comparatively slight flexure, the greater inclination of its western dip ceases; but this forms no exception to the general fact as seen in the other Appalachian folds. As would follow from the above theory in regard to their origin, it is not until they attain a considerable angular elevation that the pushing over of their summits, which is a surface movement, begins to take place.

Here, as in other parts of the Appalachian region, there is evidence of a transverse system of disturbances. An example is seen on McFarland's run of Hughes river, a few miles east of

this uplift, in a vertical fissure cutting through nearly horizontal rocks with a bearing, as described by Lesley, of 78° east of north. It is filled with a sort of solidified bitumen, or asphalt, —the result, probably, of the slow oxydation of the heavy residuum of oils which once occupied it.

ART. XLVI.—*Remarks on the Drift of the Western and Southern States, and its relation to the Glacier and Iceberg Theories;* by EUG. W. HILGARD, Ph.D., State Geologist of Mississippi.

IN a recent paper on the Quaternary formations of Mississippi (this Journal, May, 1866), I have expressed my views concerning the remarkable formation designated as the "Orange Sand," in my Report on the Geology of Mississippi.

The admirable exposition of the whole subject of the Post-tertiary formations in Dana's Manual of Geology, to which I have since been enabled to refer, as well as the portion of Dana's Address before the American Association in 1855, relating to that period, suggest to me with greater clearness the points to be settled in establishing the presumed correspondence of the Orange Sand of the Southwest to the recognized drift of the Northwest. On this subject I now propose to offer a few additional remarks.

I have not perhaps, in the paper referred to, been sufficiently explicit, in comparing the Orange Sand to the northern drift in general, without specially mentioning as its supposed congener, the drift of the Northwest, particularly that of Illinois, Wisconsin, Iowa and Missouri. That the drift of New England, as described by Hitchcock and Dana, can be satisfactorily accounted for on the glacier theory alone, few who have delved among the ancient and modern moraines of Switzerland will question.

But the Western drift deposits, even so far north as Iowa, as Hall's observations show, and still more in Illinois and Missouri, differ seriously in their structure both from the New England moraines, and from the lines of blocks apparently left by the glacier melted *in situ*. In the West the "erratic" blocks, both rounded and angular, are commonly found imbedded in deposits distinctly stratified over considerable areas, and consisting of gravel, sand, or even clay; and the pebbles accompanying them are not the irregular, scored and scratched forms of the moraine pebble, but clearly rounded by aqueous action.

Where a moraine has not, subsequently to its formation, been violently shoved forward by an unusual advance of the glacier, some semblance of stratification may occur, in consequence of great regularity in the rolling down of the detritus. But such

an appearance rarely extends connectedly beyond a few yards, nor is it likely it ever should, however stupendous the scale of the glacier. Nor will any one, acquainted with moraines and their material, be likely to mistake an aqueous deposit for a moraine, or *vice versâ*.

In Desmoines county, Iowa, according to Hall, the drift consists of "partially stratified deposits of clay, sand and gravel, with boulders of primary and secondary rocks irregularly distributed through the mass, though usually most abundant in the lower portion." These boulders in the description of Lee county are spoken of as "worn and rounded masses" of various rocks, occurring in distant localities. We have the same in Henry county; in Van Buren, this deposit furnished "a mass of siliceous wood," which "presented none of the water-worn characters of a boulder; but the angles were as sharp and well defined as if it had never been removed from the spot where it was at first buried." In Washington county, again, we find mentioned "a heavy deposit of drift material presenting the usual characteristics of this formation, and consisting of irregularly stratified beds of sand, gravel and clay, with an average thickness of from forty to sixty feet."

The above, taken *verbatim* from Hall's Iowa Report, might serve as a very fair description of a good portion of the Orange Sand of Mississippi, the only difference being the greater size of the boulders in the more northern locality; a merely quantitative variation.

In Missouri the phenomena are the same. The drift there, according to Swallow, is "a heterogeneous stratum" (but a *stratum* still!) "of sand, gravel and boulders, all water-worn fragments of the older rocks." Some of these are derived from localities as far distant as "St. Peters river, about 300 miles north of St. Joseph. But the paleozoic fragments are usually from localities near where they rest.... and are as *completely rounded* as those from more distant points."

The latter remark is interesting, as it shows precisely what I found to be the case in the eastern pebble belt of Mississippi. The pebbles there are almost exclusively derived from the siliceous group of the Carboniferous, which occurs only in patches farther northward, but underlies the pebble strata themselves (Miss. Rept., p. 17, ff.).

But Swallow goes on to say that "there are other deposits, particularly in the middle and southern parts of the State" (*sic!*) "which are not genuine drift, and yet they bear a greater resemblance to that than to any other formation, and occupy the same stratigraphical position."

This, also, is precisely the predicament of the Orange Sand deposits.

As for the drift of Illinois, I am unable to refer to the Reports of that State; but my own early recollections of it such as it exists in St. Clair county, place it in precisely the same category with the drift of Iowa and Missouri. Only I remember distinctly the occurrence in it, of *angular* boulders of syenite, greenstone and quartzite.

Nevertheless, most of the material by far, in all these cases, is *water-worn;* it is, more or less irregularly but distinctly, *stratified; no glacier scorings* are mentioned, either on the pebbles or on the adjacent rock.

The *glacier theory alone* cannot, therefore, account for these deposits north of the Ohio, any more than for the Orange Sand delta south of it. And even as far south as Vicksburg, the action of *water alone* is inadequate to account for the transportation of the boulders found in these beds.

Dana's remark (Manual, p. 554) that "while the glaciers were disappearing, many a stream or lake would have existed to stratify the drift, and cause denudation in elevated places," points no doubt to the true explanation of these phenomena: but it does not go far enough to satisfy existing facts. Agassiz has observed that "the melting snows of the declining glacier epoch" may have been instrumental in the formation of the river terraces; but Tuomey was, I believe, the first to point out, that the Southern drift may have been formed in consequence of the *sudden* melting of the northern glaciers (Second Report on the Geology of Alabama, ed. Mallet, p. 146); such as would have resulted from a first, rapid depression of so huge a mass of ice below the snow line.

The assumption of a pretty *rapid* depression seems necessary to account for the immense volumes of water required to produce the observed effects, and also to explain the possibility of inland transportation of boulders by floating ice—if indeed, after a depression of near 500 feet below the present level in the latitude of Montreal, *inland* it could be called. Though comparatively but slightly depressed at its mouth, the more immediate valley of the Mississippi would have formed a deep irregular gulf, filled with ice-cold water by the enormous influx from the glacier region forming its northern shore; whose icebergs would thus be floated southward in a medium unfit for the permanent existence of any fauna likely to be fossilized, viz: ice-cold fresh water in a state of violent flow.

That the action must at first have been extremely violent, is proved by the deep erosion of the underlying formations, and the transportation and subsequent redeposition in mass, of their materials, more or less altered; which is exhibited on so extensive a scale in Mississippi. But for the fact that wherever the

more ancient strata were readily susceptible of denudation, they have entered largely, at times almost exclusively, into the composition of the Orange Sand strata, we might suppose that the surface of the former (which appears to be at least equally as hilly as the present one) had been denuded by atmospheric agencies during the glacial period of elevation.

When, after the subsidence of the first rush, the velocity of the water had so far diminished as to render it capable of forming stratified deposits, these would naturally possess the mixed character resulting from a twofold mode of transportation—by water, and by floating ice—the former, no doubt, in many cases, succeeding the latter, and, by the grinding action of the smaller gravel and sand, transforming the angular blocks first dropped into the rounded boulders characterizing the drift of Iowa and Missouri, and faintly represented by their scattered congeners in the Orange Sand delta.

It would thus seem that the grandly simple means of a single elevation and re-depression in the northern latitudes, to which the phenomena of the ancient glaciers and sea-beaches point us, will equally satisfy the conditions required for the formation of the Western and Southern Drift. Down to the later stages of the northern re-depression, the predominant slope would everywhere be southward, so as to collect in the Mississippi valley the glacier waters, not only of the whole extent of northern territory now tributary to it, but probably also those of the present arctic slope of British America, and a portion of that now tributary to the St. Lawrence. Hence the comparative absence of stratified drift from the Northern Atlantic slope of the United States; and its presence, on the contrary, on the sea border of the Southern States, as stated by Tuomey, *l. c.*

As to the Champlain epoch, it would seem to be represented in its *later* part only, by that which in the Western and Gulf States has formed the "second bottoms." In the Mississippi Valley the formation of the latter has clearly been preceded by a period of comparative quiet succeeding the stormy times of the drift proper, and giving rise, successively, to the deposits of the Bluff or Loess, and the superincumbent Yellow Loam. These ought to be contemporaneous with the earlier stages of the Champlain epoch in New England and Canada; and their great difference of character might, it seems to me, be explained on the simple and probable supposition, that in the more southerly regions the depression took place somewhat later, even as, obviously, it was vastly less in extent. The Bluff formation of the valley of the Mississippi bears the most obvious relations to the present channels of the larger rivers, and it contains the vestiges of a fauna proving that the deluge of ice-cold water had

ceased; while yet the volume of water carried by those channels greatly exceeded that which corresponds to the era of the second bottoms, south, or to the bottom prairie of Missouri.

The only sea-beach terrace now existing on the Gulf coast, and with which the second bottoms show a manifest confluence of level and material, is from 18 to 24 feet above tide-water. But the evidences of sea-beach or tidal action extend far back into the interior (Miss. Rept. p. 29), so that in fact, the second bottom of the Pascagoula exhibits *that* structure, and not the usual one resulting from flowing water, throughout its course. The same structure, moreover, extends to some elevation into the bordering uplands, where these sands overlie the Post-pliocene beds and Orange Sand.

The present beach terrace of the gulf coast cannot, therefore, be considered as the true measure of the amount either of depression during, or re-elevation subsequent to, the Champlain epoch. Whether the great absolute elevation of some of the Orange Sand ridges of Mississippi and Alabama (probably exceeding in some cases, 700 feet) necessitates the assumption of even a greater depression than the present beach-marks would indicate, the known facts are hardly adequate to determine.

University of Mississippi, July 12, 1866.

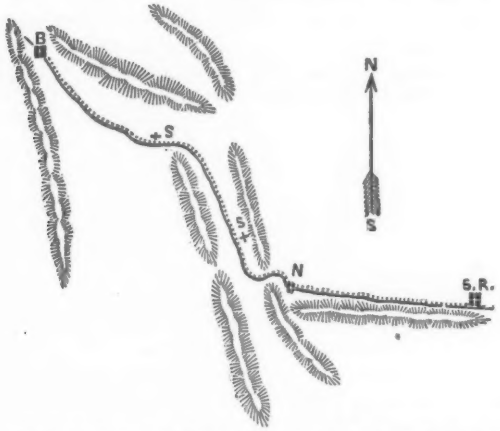
ART. XLVII.—*New locality of Meteoric Iron in Cohahuila, Northern Mexico;* by CHARLES UPHAM SHEPARD.

FOR my knowledge of this most remarkable locality I am indebted to the following communication of my friend Prof. Forrest Shepherd of New Haven, Conn., who, twenty-seven years ago, furnished me the earliest notice of the fall of a meteoric stone at Little Piney, Mo. The weightiness of the present contribution makes ample amends for so protracted a silence.

"*My dear Sir:*—Remembering your untiring efforts in meteoric research, I take pleasure in forwarding to you a specimen broken from the immense mass of native iron protruding from the earth in Northern Mexico, northwest from Santa Rosa, of the department of Cohahuila. This I am enabled to do through the kindness and perseverance of my esteemed friend Maj. E. M. Hamilton, who, with Col. David Branson and Capt. Charles W. Dey, accompanied me in my late visit to the silver mines of Nueva Leon, and to each of whom I am indebted for protection and numerous kind offices.

The route pursued by Maj. Hamilton was as follows (see map):

From Santa Rosa westerly to Naciemento, 40 miles, more or less. From Naciemento westerly or northwesterly about 15 miles to a gap through the mountains, called Puerta Santana. From the Puerta, northerly for 60 miles, along the valley, past a spring to the termination of the mountain on the left; and thence



B, Bonanza; N, Naciemento; P, Puerta Santana; S, Springs; S. R., Santa Rosa.

around the end of the mountain, passing a second spring, by a northwest route for a distance of about 50 miles toward an apparent junction of the mountains, and where the valley becomes narrow, to an open space about one-quarter of a mile square, partially overgrown by palmetto palms. Here might be seen fourteen ponderous masses of native iron, the largest of which rises upwards of four feet above the ground, having the form of a bee-hive—being five feet in diameter where it enters the soil, and into which it descends for an unknown depth. Major Hamilton informed me that he excavated on one of its sides to the distance of eighteen inches, without discovering any diminution in its diameter.

"Hoping that this remarkable locality may hereafter shed some additional light on the mysteries of aërolitic origin,

"I remain very truly yours, FORREST SHEPHERD."

As the above communication does not give any name for the locality, it seemed requisite to bestow one, out of regard to convenience of reference in our meteoric catalogues. I have therefore affixed to it the designation *Bonanza*, upon Prof. Shepherd's map.

I am unable to decide whether these iron masses have been

described by others, though it may prove they are the same as those mentioned by Assistant A. Schott on page 34, part ii, of Major Emory's Report on the Mexican Boundary Survey, and which are laid down as occurring 90 miles northwest from Santa Rosa; but whether he refers to the Santa Rosa of Cohahuila, lat. 28°, long. 101° 30′, is not certain. They certainly do not seem to belong to the locality on the *Sancha estate*, from whence came the 250 pound mass in the Smithsonian Museum at Washington, as this is stated to have occurred but 50 or 60 miles from Santa Rosa, Cohahuila.

Description of the specimen.

It is a flattened cleavage mass rather above a quarter of an inch in thickness, by two inches in length and one and a half broad. It weighs 120 grams, or rather more than a quarter of a pound. Its color is dark iron-black without any appearance of iron-rust. About three-fourths inch square of one of its sides exhibits the original surface of the mass, showing three well marked though shallow polyhedral depressions, but, like the Madoc, the Texas, and many other irons, without any well defined crust. For the rest, the two broad surfaces are nearly plane, even and parallel, and evidently came from a natural cleavage in the mass. The edges, except where recently broken, are also plane, and have resulted from octahedral cleavages.

A very distinct cleavage shows itself also about midway between the two broad surfaces; and taking advantage of this through the slight projection of a lamina rather more than one-eighth of an inch in thickness, a fragment weighing six grams was easily broken with the aid of a vise and a few slight blows of the hammer. The separation brought to light the most perfect cleavage I have yet seen among meteoric irons, as also the most splendent and silvery luster. Experiments upon other portions of the mass made it likewise apparent that throughout, it belonged to one individual crystal, like similar fragments from the great Orange river, the Braunau, and the Putnam irons. No lamina of a large crystal of blende or fluor could present a more homogeneous or brilliant cleavage than belongs to this entire specimen; nor should I be surprised if in this respect, it is other than a microcosm of the parent block.

About half a gram was treated with dilute nitric acid. It was readily attacked, and after twenty minutes was dissolved, with the exception of a slight pulverulent or granular residue, which, on being cleared from the solution, washed and thrown upon a filter, was seen to consist of short, glittering, needle-like points, almost too minute to be discerned without the assistance of a lens. Among them was a feeble trace of a blackish powder, proceeding no doubt from the black pellicle of the exterior

of the mass. The brilliant capillary points, when more nearly examined, seemed identical in size and character with the rhabdite crystals of Reichenbach, so beautifully figured in Prof. Rose's admirable memoir on meteorites,[1] as occurring in the Braunau meteoric iron. The lens also shows upon the cleavage planes of the iron, those peculiar striæ which Prof. Rose has pointed out as occurring in the Braunau iron; in particular, such as are parallel to the edges and certain diagonals of the cube, and which Prestel has shown to be present also in cleavage crystals of artificial iron. So far therefore as one sample can show, the structural identity of this iron with the interesting Braunau mass is very clear.

The specific gravity, determined by a single experiment, was 7·50. The solution in nitric acid, when precipitated by ammonia, gave the blue color characteristic of nickel.

I shall avail of an early opportunity for having a portion of the surface polished and etched; as also for making a fuller investigation in respect to its chemical constitution.

In this extraordinary occurrence of metallic meteorites, we are again forced to reflect upon the singular manner in which they follow great mountain chains, at least on the American continent. Is their preponderance in such regions at all explained by the greater impenetrability of the surfaces upon which they have fallen?

Amherst College, Sept. 14, 1866.

ART. XLVIII.—*On the Spectra and Composition of the Elements;* by Prof. GUSTAVUS HINRICHS, Iowa State University.

§ 1. Two years ago I communicated to this Journal the results of a *preliminary* investigation of the distribution of the dark lines in the spectra of some of the elements, especially the spectra of the alkaline earths and iron.[2] At that time no exact and comprehensive determinations of the *wave-lengths* corresponding to these lines had been made; our investigation was therefore necessarily based upon Kirchhoff's arbitrary millimeter scale. As shown by us, this scale will answer very well for a limited range of the spectrum—but if distant parts of the spectrum are to be compared, this scale becomes useless, and the absolute wave-lengths are required. We, for this purpose, made use of Fraunhofer's principal lines and a geometrical interpolation—but these points of comparison are evidently too few to warrant great accuracy.

[1] Beschreibung und Eintheilung der Meteoriten auf grund der Sammlung im mineralogischen Museum zu Berlin, von Gustav Rose. Berlin, 1864.

[2] This Journal, 1864, vol. xxxviii, pp. 31–40.

Having now some direct determinations of wave-lengths, we resume our investigation. It will be seen that the results of our preliminary investigation are fully confirmed in their essential features, and that the number of elements to which they apply is greatly enlarged.

§ 2. The basis for the present investigation are the determinations of Plücker and Ditscheiner. The results only of Plücker's determinations are accessible to us;[2] they apply to the metalloids, thus enabling us to extend our investigations to these elements. Ditscheiner[3] has determined the wave-length of a great number of the lines laid down in Kirchhoff's map of the spectrum, thereby enabling us by a simple geometrical interpolation to find the wave-length of any of Kirchhoff's lines with great accuracy, the number of lines determined by Ditscheiner being sufficiently extensive to warrant the use of a large scale in this interpolation. The scale we used was 1 mm. of Kirchhoff's scale and one ten-millionth *millimeter wave-length*, severally equal to two millimeters. On this scale the length of the spectrum from B to G, or from 600 mm. to 2800 mm. Kirchhoff, taken as abscissæ, becomes 4400 mm., or nearly 15 feet. In some instances we used a five times longer scale, making the spectrum 75 feet.

There is still another series of determinations by Angström,[4] but the original memoir is not accessible to us, and the extract in this Journal does not admit of direct comparison with Kirchhoff's scale.

§ 3. We will now first give the extension of our laws to the spectra of the metalloids on the basis of Plücker's determinations; then review the spectra of the alkaline earths and iron by means of Ditscheiner's measurements; and in conclusion make a few theoretical remarks suggested in the course of our investigation.

In the following, W will always denote the *wave-length* expressed in *ten-millionths of a millimeter*, D the differences, *i* the assumed number of equal intervals, W′ the calculated wave-lengths, E the error, the difference between observation and calculation, E=W−W′ and *d the interval*.

§ 4. *Hydrogen spectrum.*

Line.	W	D	*i*	W′	E
α	6533			6533	0
		1690	10		
β	4843			4843	0
		504	3		
γ	4339			4336	+3

where $d=169$. Range from red to indigo, or C to G.

[2] This Journal, 1865, vol. xxxix, pp. 217–18.

[3] Bestimmung der Wellenlänge der Fraunhofer'schen Linien des Sonnenspectrum. Sitzungsberichte der Akad. d. Wiss. Wien, 1864, Bd. 50, pp. 296–341. This Journal, 1866, vol. xli, pp. 395–396.

[4] This Journal, 1865, xxxix, 215.

§ 5. *Chlorine spectrum.*

Line.	W	D	i	W′	E
α	5451			5451	0
		235	5		
β	5216			5216	0
		424	9		
γ	4792			4793	+1

where $d=47$. Range, from yellow to blue.

§ 6. *Bromine spectrum.*

Line.	W	D	i	W′	E
α	5169			5166	+3
		376	15		
β	4793			4791	+2
		27	1		
γ	4766			4766	0
		75	3		
δ	4691			4691	0

where $d=25$. Range, from green to blue.

It will be noticed that the first interval is five times the last.

§ 7. *Iodine spectrum.*

Line.	W	D	i	W′	E
α	?			?	
β	5947			5949	−2
γ	?	610	77	?	
δ	5337			5333	+4
		170	21		
ε	5167			5165	+2
		506	63		
ζ	4661			4661	0
		32	4		
η	4629			4629	0
		183	23		
θ	4446			4445	+1
		231	29		
ι	4215			4213	+2

where $d=8$. Range, from orange to indigo.

Though the differences E are not quite so insignificant as in the preceding, they are yet within the errors of observation; and though the intervals seem to follow no law, yet they may be grouped as follows:

	77	21	63	4	23	29
					56	
or	77	21	63			
or	11	3	9		8 times 7	
		12			8	
or	11					
very nearly		3 : 3 : 2,				

and $4+23=27$ with 29 forms another group if an intermediate line at 28 should be observed. As it seems, 7 is divisible in most of these intervals, $7.d=56$ will be an interval of a higher order.

§ 8. *Nitrogen spectrum.*

Line.	W	D	i	W′	E
β	6610			6609	+1
		521	8		
11	6089			6089	0
		327	5		
17	5762			5764	−2

where $d=65$. Range, from red to yellow.

§ 9. *Oxygen spectrum.*

Line.	W	D	*i*	W′	E
α	6150			6150	0
		822	6		
β	5328			5327	−1
		143	1		
γ	5185			5190	+5
		818	6		
δ	4367			4367	0

where $d=137\frac{1}{3}$. Range, from orange to near indigo.

§ 10. *Mercury spectrum.*

Line.	W	D	*i*	W′	E
α	5782			5784	−2
		23	1		
α′	5759			5759	0
		298	12		
β	5461			5459	+2
		102	4		
γ	4359			5359	0

where $d=25$. Range, from yellow to near indigo.

In the synopsis of Plücker's determinations given in this Journal (vol. xxxix), we find also the lines of several compounds, and we will improve this opportunity to test the application of the law of multiple distances to the four binary compounds investigated by Plücker.

§ 11. *Terchlorid of phosphorus spectrum.*

Line.	W	D	*i*	W′	E
α	6493			6492	+1
		469	13		
β	6024			6024	0
		433	12		
γ	4591			4592	−1

where $d=36$.

§ 12. *Bichlorid of silicon spectrum.*

Line.	W	D	*i*	W′	E
α	6329			6329	0
		358	3		
β	5978			5980	−2
		928	8		
γ	5050			5050	0

where $d=116\frac{1}{3}$.

§ 13. *Bichlorid of tin spectrum.*

Line.	W	D	*i*	W′	E
α	6445			6444	+1
		651	65		
β	5794			5794	0
		210	21		
γ	5584			5584	0
		251	25		
δ	5333			5334	−1
		809	81		
ε	4524			4524	0

where $d=10$.

§ 14. *Carbonic acid spectrum.*

Line.	W	D	*i*	W′	E
γ	5599			5600	−1
		409	41		
δ	5190			5190	0
		689	69		
ζ	4501			4500	−1
		119	12		
η	4382			4380	+2

where $d=10$.

§ 15. The preceding tables may be condensed to the following:

Substance.	d	Values of i.						
Hydrogen,	169	10	3					
Chlorine,	47	5	9					
Bromine,	25	15	1	3				
Iodine,	8	77	21	63		4	23	29
		or 77	: 84	:			56	
		or 11	: 12	:			8	
		or $2\frac{1}{12}$	: 3	:			2	
Nitrogen,	65	8	5					
Oxyen,	$137\frac{1}{8}$	6	1	6				
Mercury,	25	1	12	14				
Terchlorid of phosphorus,	36	13	12					
Bichlorid of silicon,	$116\frac{1}{8}$	3	8					
Bichlorid of tin,	10	65	21	25	81			
Carbonic acid,	10	41	69	12				

§ 16. It can hardly be assumed that the determinations of Plücker are as accurate as those of Ditscheiner; the latter are, as will be shown, not always reliable in the last figure. Hence the agreement between observation and our calculation is in most cases astonishingly close; thus for hydrogen, where $d=169$ there is only one single deviation, and this amounts to only three units; the chlorine spectrum shows as good as no deviation at all, only one unit for $d=47$ on a range of 700. It is similar for bromine, oxygen, nitrogen, and mercury. Only iodine deviates for one single line (δ) more than is proper, especially as d is only 8; but then this being the only instance, it may be due to a greater error in the observation of δ.

It may perhaps be objected, that only few lines are taken for each element. But we have taken *all* lines for which determinations could be found, and though they may be few in number, they are ranging through the greater part of the spectrum.

Assuming that the other metalloids will correspond in this as they do in all other respects, we may express the above by the following law: *the wave-lengths of the dark lines in the spectra of the several metalloids differ from one another by simple multiple of a certain number, d, peculiar for each element*, and the spectra of the binary compounds given do not show so simple multiples, but the differences are yet expressible in multiples of a certain number for each compound.

The above law is substantially the same as the laws deduced in the preliminary investigation.

§ 17. In all the foregoing tables we have made use of Plücker's observations, which are only known to us in their final result. The following values of W will be taken from Ditscheiner, and as his memoir is at hand, we may first ascertain the degree of accuracy of his observations.

For Kirchhoff's line 1834·0 he finds W=5040·0 from 13 determinations, ranging from 5039·3 to 5041·4, showing a range of 2·1 (page 326, l. c.).

For K=1655·6 he finds W=5165·8 from 13 determinations, ranging from 5165·1 to 5166·8 or a range of nearly two of our units (page 325).

In accordance herewith we found it impossible for small parts of the spectrum, to draw a straight line through the points determined according to Ditscheiner's wave lengths as ordinates to Kirchhoff's millimeters as abscissæ; showing that the decimal of Ditscheiner could not be relied upon, and in a few instances even the unit was uncertain.[1] We will now review the *calcium* spectrum, as given in our first article. In addition to the preceding, I will signify the intensity as given by Kirchhoff, and K the place on Kirchhoff's millimeter scale. W is obtained as before stated, either directly from Ditscheiner's table, if the line itself was measured by him, or by geometrical interpolation, having carefully constructed the neighboring part of the spectrum—always making a separate drawing for each group of lines according to the scale mentioned in § 2. It was impossible to obtain reliable results by arithmetical interpolation, the small errors of observation thereby affecting the final result to a very great extent, while by proper drawing of the line the individual errors of observation were as good as eliminated.

§ 18. *Calcium spectrum.*—We will, as in our first article, consider each of the six principal groups separately.

GROUP I. d_1=3·3. Near line G.

I	K	W	D	*i*	W′	E
5*c*	2869·7	4322·0			4322·0	·0
			13·2	4		
4*b*	2864·7	4308·8			4308·8	·0
			6·5	2		
4	2854·7	4303·2			4302·2	+·1
			3·3	1		
4*c*	2834·2	4299·0			4298·9	+·1

The intervals are just as given in the preliminary investigation—and E is completely within the errors of observation.

Group II contains too few lines.

GROUP III. d_2=1·16. Near line E.

I	K	W	D	*i*	W′	E
4*b*	1533·1	5261·5			5261·48	+·02
			2·1	2		
4*b*	1532·5	5261·7				
4*c*	1530·2	5263·6			5263·80	−·20
			1·3	1		
5*c*	1528·7	5264·9			5264·96	−·06
			4·7	4		
6*c*	1522·7	5269·6			5269·6	·00

The scale used for this interpolation was three times the one given in § 2; 1 mm. K was represented by 6 mm.

[1] In order to prevent misunderstandings it must be remembered that our unit is the ten-millionth of a millimeter, while Ditscheiner expresses his results in millionths of a millimeter.

The difference between the first two lines is too small to warrant any conclusion drawn from the same.

GROUP IV. $d_4 = 1{\cdot}46$. In the yellow.

I	K	W	D	*i*	W′	E
3*d*	1235·0	5582·0			5582·00	·00
			5·7	4		
4*c*	1229·6	5587·7			5587·84	−·16
			1·7	1		
2*d*	1228·3	5589·4			5589·30	+·10
			4·2	3		
5*d*	1224·7	5593·6			5593·68	−·08
			3·9	2		
5*d*	1221·6	5597·5			5596·60	−·10
			2·5	2		
3*c*	1219·2	5600·0			5599·52	+·42
			1·0	1		
5*d*	1217·8	5601·0			5600·98	+·02

These intervals form the following simple series:

	4	1	3	2	2	1
or	4	4		2	2	1
or	4	4		4		1

GROUP V. $d_5 = 6{\cdot}57$. In the orange.

I	K	W	D	*i*	W′	E
2*e*	894·9	6102·0			6102·0	·00
			19·0	3		
4*b*	884·9	6121·0			6121·71	−·71
			40·0	6		
5*b*	863·9	6161·0			6161·13	−·13
			6·7	1		
3*d*	860·2	6167·7			6167·70	·00

GROUP VI.

There are too few observations for a very accurate determination of the corresponding wave-lengths of this group in the red part of the spectrum. The difference of 2·3 K as found in our preliminary investigation corresponds to 6·23 wave-length in this part of the spectrum.

Comparing the different groups we need not expect the difference *d* itself to be the same for all groups; but such a multiplum thereof as the intervals indicate, ought to be the same. Thus we find

Group I,	$d_1 = 3{\cdot}3$	$2d_1 = 6{\cdot}60$
" III,	$d_3 = 1{\cdot}16$	$6d_3 = 6{\cdot}96$
" IV,	$d_4 = 1{\cdot}46$	$4d_4 = 5{\cdot}84$
" V,	$d_5 = 6{\cdot}57$	$1d_5 = 6{\cdot}57$
" VI,	$d_6 = 6{\cdot}23$	$1d_6 = 6{\cdot}23$
	the mean of which is	$d = 6{\cdot}44$

As far as the above numbers go, we see that the interval approaches equality, but is not strictly so. Yet it may well be borne in mind that the greatest deviation either above or below the given mean value amounts to only half a unit; so that though the equality of the interval cannot be considered as demonstrated, yet the inequality is not demonstrated either.

It is now necessary to consider the single lines given by Kirch-

hoff outside of the preceding groups. These give the following result: $d=16$.

I	K	W	D	i	W′	E
2*c*	1832·8	5041·0			5041·0	0·0
			144·6	9		
5*b*	1627·2	5185·6			5185·0	+·6
			159·6	10		
2*b*	1443·5	5345·2			5345·0	+·2
			511·1	32		
3*c*	1029·3	5856·3			5857·0	−·7
			864·9	54		
2*b*	641·0	6721·2			6721·0	+·2

Introducing the groups, we obtain, always $d=16$.

Group.	I	K	W	i	W′	E
I.	5*c*	2869·7	4322·0		4321·0	+1·0
				7		
II.	5*c*	2638·8	4434·0		4433·0	+1·0
				38		
	2*c*	1832·8	5041·0		5041·0	0·0
				9		
	5*b*	1627·2	5185·6		5185·0	+ ·6
				$4\frac{3}{4}$		
III.	4*b*	1533·1	5261·5		5261·0	+ ·5
				$5\frac{1}{4}$		
	2*b*	1443·5	5345·2		5345·0	+ ·2
				16		
IV (last).	5*d*	1217·8	5601·0		5601·0	0·0
				16		
	3*c*	1029·3	5856·3		5857·0	− ·7
				19		
V.	2*e*	894·9	6162·0		6161·0	+1·0
				$20\frac{3}{4}$		
VI.	2*c*	720·1	6492·0		6493·0	−1·0
				$14\frac{1}{4}$		
	2*b*	641·0	6721·2		6721·0	+ ·2

The series of intervals admits of the following contractions:

7	38	9	$4\frac{3}{4}$	$5\frac{1}{4}$	16	16	19	$20\frac{3}{4}$ $14\frac{1}{4}$
7	38	9	10		16	16	19	35
45		9	10		16	16	54	
	54		10		16	16	54	
		64			16	16	54	
or 8 times		8			2	2	7	

Finally it must be noticed that this difference, $d=16$, according to this table may be taken an even number of times, up to 8, so as to give a physical interval; and $2d=32$ is 5 times 6·4, the mean of the intervals deduced for the several groups.

Thus we believe to have shown that the wave-lengths of Ditscheiner in regard to the regular distribution of the dark lines in the calcium spectrum fully confirm the laws enunciated in our preliminary investigation.

§ 19. *Barium spectrum.*—Having discussed the calcium spectrum at considerable length, we may pass more lightly over the barium spectrum. As there are very few barium lines coincident with those measured by Ditscheiner it may be sufficient to multiply the difference between Kirchhoff's numbers with the tangent or *grade* of the curve at the point considered (that is, the ratio between dW and dK) as taken from our drawings.

I	K	D	Grade.	D′		
2*c*	2031·1					
		41·6	·8	33·3	=	2×16·65
6*c*	1989·5					
1*c*	1287.5					
		13·3	1·25	16·6	=	1×16·6
3*b*	1274·2					
2*a*	1083·0					
		51·2	1·3	66·6	=	4×16·65
2*a*	1031·8					
		41·6	1·6	66·5	=	4×16·64
1*b*	890·2					
		15·9	1·9	30·2	=	2×15·1
4*b*	874·3					
3*a*	719·6					
		·9	2·5	2·3	=	$\frac{16·1}{7}$
2	718·7					

On account of our method here used, only such lines could be considered as are sufficiently close to admit of our reduction of the difference in K by simple multiplication with the grade. All lines accessible give the interval 16·6—the mean of the six determinations is 16·3. It is also seen that the actual intervals are very simple multiples hereof.

§ 20. *Magnesium spectrum.*

I	K	W	*i*	W′	E
6*e*	1655·6	5165·8	1	5166·07	−·27
6*f*	1648 8	5171·3		5171·3	·0
4*c*	1634·7	5181·0	2		
4*g*	1634·1	5181·5		5181·76	+·26

where $d=5·23$.

The line 6*f* is Fraunhofer's *b*. The reduction was here effected arithmetically by means of the grade ·8; affecting only the last given number.

These three or four lines forming a natural group, the total range or 3*d*, i. e., 15·69, becomes characteristic of this element.

§ 21. *Strontium spectrum.* It will be noticed that this difference is very nearly the same as the one previously found for barium and calcium. It thus becomes of great interest to ascertain whether or not strontium also herein agrees with the other members of the group of alkaline earths. We have for strontium

I	K	W	*i*	W′	E
3*b*	753·8	5400·0	54	5400·0	0·0
3*a*	1274·7	5532·0		5530·6	+1·4
4*c*	1320·6	5480·0	3	5482·3	−2·3
3	2857·9	4306·0	73	4307·0	−1·0
4*a*	2858·5				
2	2858·9				
	2859·4				

where $d=16·1$.

The intervals give

		54		3		73
	or	18	:	1	:	$24\frac{1}{3}$

or the first to the last nearly as 3 : 4. Also 54+3=57 to 73 nearly as 7 : 9. The reductions were performed by means of the known grade, no one of the above strontium lines coinciding with Ditscheiner's lines.

§ 22. *The alkaline earths* give as the difference of wave-lengths characterizing their spectrum, magnesium 15·7, calcium 16·0, strontium 16·1, and barium 16·3, or in millimeters.

	Atomic weight.	Wave-difference. mm.
Magnesium, - - - - -	12	0·00000157
Calcium, - - - - - -	20	0·00000160
Strontium, - - - - -	43·8	0·00000161
Barium, - - - - - -	68·5	0·00000163
Mean value,		0·00000160

This coincidence is very remarkable, though the absolute identity can hardly be considered admissible. We shall come back to this point in our theoretical remarks.

§ 23. *Sodium spectrum.*—Professor Cooke of Cambridge has recently given[6] four drawings of the three sodium lines forming Fraunhofer's D. From these figures I find by measurement $D_1 a = 12$ mm., $a D_2 = 8$ mm., or the distances are as 3 : 2.

I am very sorry that I have not the determinations[7] of Wolf and Diacon in regard to the spectra of the alkalies. Interesting and valuable results might no doubt be derived from these observations.

§ 24. *Iron spectrum.*—We will now review the three large and beautiful groups of the iron spectrum as given in our first article.

Group I. $d_1 = 3{\cdot}29 = 0^{mm}{\cdot}000000329$.

I	K	W	i	W′	E
4*b*	1200·6	5623·25		5621·58	+1·67
5*g*	1207·3	5615·00	2	5615·00	0·00
5*d*	1217·8	5602·00	4	5601·84	+ ·16
5*d*	1231·3	5585·65	5	5585·44	+ ·21
4*a*	1239·9	5572·65	4	5572·28	+ ·37
6*c*	1242·6	5569·40	1	5568·99	+ ·41
4*d*	1245·6	5565·65	1	5565·70	− ·05

The intervals are very simple. Only the first value of E may be considered important—may disappear upon the observation of a line between the first two given above.

The intervals give the following series:

$$\begin{array}{lccccc} & 2 \quad 4 & & 4 \quad 1 & \\ & \underbrace{\qquad} & & \underbrace{\qquad} & \\ \text{or} \quad 1+5 & & 5 & & 5 & 1 \end{array}$$

showing that the following may be considered as intervals of higher order:

$$D_1' = 4d_1 = 13{\cdot}16, \qquad D_1'' = 5d_1 = 16{\cdot}45$$

[6] This Journal, 1866, xl, 179.

[7] This Journal, 1863, xxxv, 414.

GROUP II. $d_2 = 0{\cdot}9 = 0^{mm}{\cdot}00000009$.

I	K	W	i	W′	E
4*d*	1337·0	5461·8	8	5461·3	+·5
6*c*	1343·5	5454·5	10	5454·1	+·4
5*d*	1351·1	5445·3	2	5445·1	+·2
5*b*	1352·7	5443·0	12	5443·3	−·3
5*b*	1362 9	5432·5	5	5432·5	0·0
6*d*	1367·0	5428·2	7	5428·0	+·2
5*b*	1372·6	5422·0	9	5421·7	+·3
4*c*	1380·5	5413·6	5	5413·6	0·0
4*c*	1384·7	5408·9	5	5409·1	−·2
6*c*	1389·4	5404·4	1	5404·6	−·2
5*d*	1390·9	5403·7	9	5403·7	0·0
5*c*	1397·5	5395·7	5	5395·6	+·1
4*c*	1401·6	5391·0	10	5391·1	−·1
4*c*	1410·5	5381·9	14	5382·1	−·2
6*c*	1421·5	5369·8	1	5369·5	+·3
5*b*	1423·0	5368·5	3	5368·6	−·1
5*b*	1425·4	5366·0	3	5365·9	+·1
5*b*	1428·2	5363·2		5363·2	0·0

The agreement is very close. The intervals are apparently not *very* simple, but they give

8 10 2 12 5 7 9 5 5 1 9 5 10 14 1 3 3

8 12 12 12 9 10 10 5 10 15 6

first multiple of 4 (viz., 8, 12, 12, 12) and in the latter half multiples of 5 (5, 5, 10, 5, 10, 15). But we may also group these as follows:

8 10 35 5 5 10 5 10 15 6

which are all, excepting extremes, multiples of 5; the extremes would by completion with adjacent parts not observed by Kirchhoff no doubt complete the series. Dividing by the common 5 we obtain the ratios

2 7 1 1 2 1 2 3

which again may be combined as

9 1 3 3 3

To the common factor 5 above corresponds an interval of $D_2 = 5d_2 = 4{\cdot}5$; to this further interval of 3 such corresponds an interval of $D_2' = 3 \times 5d_2 = 15d_2 = 13{\cdot}5$. Both of these numbers have theoretical importance.

GROUP III. $d_3 = \cdot 709 = 0^{mm} \cdot 0000000709$.

I	K	W	i	W′	E
5*c*	2001·6	4921·5		4921·6	−·100
6*d*	2005·2	4918·8	4	4918·764	+·036
6*c*	2007·2	4917·3	2	4917·346	−·046
6*c*	2041·3	4890·4	38	4890·404	−·004
6*b*	2042·2	4889·8	1	4889·695	+·105
6*c*	2058·0	4876·4	19	4876·224	+·176
5*c*	2066·2	4870·0	9	4869·943	+·057
5*c*	2067·1	4869·2	1	4869·234	−·034
6*a*	2082·0	4857·7	16	4857·890	−·190

E as good as nothing. The intervals arrange themselves as follows:

4 2 38 1 19 9 1 16

40 20 10 16

4 : 2 : 1

Of course, the extremities not necessarily representing full intervals, need not correspond; yet they are divisible by 4. The natural intervals of a higher order in this group are: $D_3' = 4d_3 = 2 \cdot 836$, $D_3'' = 5d_3 = 3 \cdot 545$, $D_3''' = 10d_3 = 7 \cdot 090$, and perhaps $D_3^{iv} = 9d_3 = 6 \cdot 381$.

Comparing the different groups we see that the differences d are not the same, for

$$d_1 = 3 \cdot 29, \qquad d_2 = 0 \cdot 9, \qquad d_3 = 0 \cdot 709,$$

but we have for the intervals D

yellow, $D_1' = 4d_1 = 13 \cdot 16$
green, $D_2' = 15d_2 = 13 \cdot 5$
blue, $2D_3''' = 20d_3 = 14 \cdot 18$
} mean 13·61,

which are not quite equal, but good approximations. We shall come back to this difference between the values—it seems that the values are a little larger in the blue than in the red.

In looking back upon the complicated groups of the rich iron spectrum we cannot help being convinced that the dark lines are distributed according to law, at *multiple* intervals as measured by the wave-length.

§ 25. Before concluding this part of our investigation we will compare our new results with those in our first article. In the *iron spectrum*, the first group shows the following intervals:

then	8	12	17	11	3	4
now	2	4	5	4	1	1

while the second groups gave

then	8	9	2	12	5	7	10	5	6	2	8	5	9	14	2	3	3
now	8	10	2	12	5	7	9	1	5	1	9	5	10	14	1	3	3

and the third group

then	4	2	38	1	18	9	1	17
now	4	2	38	1	19	9	1	16

By having more accurate data, the intervals become more simplified, especially in regard to the consolidating of groups of lines, as has been shown in the preceding paragraph.

In the synopsis of the calcium spectrum given on page 35 of our first article (this Jour., vol. xxxviii) the deviations amount to fully

$$\alpha = 0^{mm}\cdot 0000030,$$

while in our present article the greatest deviation is only (§ 18)

$$\alpha' = 0^{mm}\cdot 0000001$$

or $\alpha : \alpha' = 30 : 1$, only one-thirtieth of the previous one.

This comparison might be considerably enlarged—but the preceding is enough to show that far from disappearing by closer approximation and more accurate data, the laws there enunciated are becoming even more prominent and unmistakable in proportion as the data of observation have become more rigorous and reliable. It may therefore be hoped that continued investigation will tend to the better establishment of the laws referred to.

And it is in view of this greater confidence we are permitted to bestow on the regular distribution of the dark lines that we will venture upon the dangerous ground of a theoretical explanation of these wonderful facts and mysterious laws. The importance of spectral analysis in a practical point of view is established; its power and use in the laboratory is known and admired; its application to, or rather its creation of, *cosmical chemistry* is one of the greatest triumphs of physical science; but I believe that the day is not distant when spectral analysis will lay open to our view the chemical constitution of the *elements*, as I have already intimated in the concluding remarks of my first article on the dark lines. We will now try to give a little detail on this difficult and obscure, but most important point.

§ 26. *Origin of the dark lines.*—The mere aspect of the dark lines in any spectroscope conveys almost a moral conviction to the mind that they are allied in their origin to the dark lines of interference produced by thin plates and the like; for example, Talbot's interference phenomena produced by the interposition of a little piece of glass or mica between the eye and the ocular of a telescope directed to a spectrum, or Baden Powell's lines produced by a glass plate immersed in a prismatic trough filled with cassia-oil, etc.

This idea is already old, and I know of no definite refutation thereof. The lines produced by the vapors of iodine, bromine, hypochlorous acid and others, are closely allied to the regular spectral lines and the above lines of interference. Böttger[*]

[*] This Journal, 1863, vol. xxxv, p. 414.

states furthermore, that the *selenium* spectrum contains between the yellow and the violet *a very large number of equidistant dark lines.*

We have furthermore proved that though not *all* lines are present, corresponding to each successive equal interval, still all the known lines occupy positions in exact accordance with the equal intervals, that is, *the dark lines are as if from the whole number of exactly equidistant lines some had been blotted out or at least obscured by other causes.*

From all this, both the experimental and our theoretical evidence, we conclude, that *the dark lines are produced by interference.* We will now see whether we can account for some other peculiarities of their distribution by means of this hypothesis.

Erman, in his investigation of the absorption lines of iodine, bromine, etc., has shown that such lines produced by a plate of mica are the closer together, the thicker the plates, and that they are farther apart in the violet than in the red.

Considering matter to consist of solid atoms kept at certain distances from each other, we may either take the *distance* or the *dimension* of the atom as represented by d. But if d represented the distance, then the lines ought to change with the temperature, becoming more numerous with higher temperature—the dark lines would be as changing as the arrangement of the atoms. But we know the spectral lines to be permanent and immutable, as we suppose the atoms themselves to be.* We therefore must consider the dimensions of the atoms to correspond to d.

The atoms may be considered as having at the most three different dimensions, so that accordingly *the dark lines in the spectra of the elements are the result of the interference of at most three systems of interferences determined by the three dimensions of the atoms.*

We shall now compare some of the points involved herein to the results of observation laid down in the preceding paragraphs.

§ 27. *Is the difference constant* throughout the entire spectrum? or are the lines farther apart in the blue end of the spectrum, where the wave-length is smaller? The values of D obtained for the iron groups are: D in the yellow 13·16, in the green 13·5, in the blue 14·18; showing a slight increase in the same direction as demanded by Erman's formula. But the difference is too minute to admit of positive decision. Further and still more extended research is required. But we may safely affirm that in case any such variation is actually proved it will be found to be very small.

§ 28. *The lines of the same group are equidistant*—as required by this theory; for we have seen the same difference d to express all the numerous observed values for even very extensive

* Mitscherlich's observations in regard to the spectrum of iodine admits of other explanation.

groups of lines. Thus iron group I numbers 18 lines ranging through nearly 100 mm. of Kirchhoff's scale. As observed before, not all lines are actually observed—but that may be due to the coëxistence of the several systems caused by the several dimensions of the atoms, whereby also a variation in the intensity of the lines would be produced.

§ 29. *Influence of the dimension of the atoms.*—The greater d, the closer must be the dark lines. If this theoretical result be applied to the elements, considering their dimension as d, it would follow, if we adopt the hypothesis of one element, thereby measuring volume by the weight of the atom, that *the lines must be generally the closer the greater the atomic weight of the elements.*

In the following table A is the atomic weight taken from Will's Jahresbericht for 1863 as copied in the Smithsonian Report for 1864; d is the interval as found in the preceding paragraphs:

Metalloids.	A	d	Metals.	A	d
Hydrogen,	1	169·0	Magnesium,	12	5·23
Chlorine,	35·5	47·0	Calcium,	20	3·3–1·16
Bromine,	80·0	25·0	for several groups,		1·46–6·57
Iodine,	127·0	8·0			6·23
Nitrogen,	14·0	65·0	Iron,	28	d_1=3·29
					d_2= ·9
Oxygen,	8·0	137½			d_3= ·709
			Mercury,	100	25·0

We notice first the great intervals of the metalloids as compared to those of the metals; also in general a greater interval for smaller atomic weight. This is the more conclusive for similar elements, as exemplified in the chlorine group.

We can not expect a more close agreement, for the dimensions and not the *volume*, proportional to the atomic weight, decide the distribution of the dark lines.

But the dimensions can only be found by for a moment accepting our hypothesis of the construction of the elements. We hope that the preceding will induce the reader to approach this question.

§ 30. *Dimensions of the atoms.*—We suppose all elementary atoms to be built up of the atoms of *one single matter*, the *urstoff*. Let the atomic weight of hydrogen referred to this prime-element be H—we have reason to believe that H=4.

Whatever be the form of these atoms, the laws of mechanics force them to arrange themselves regularly—and the most stable form will be the prism. If quite rectangular, and a, b, c be the number of primary atoms, in the three directions, we shall have (leaving out the factor H)

$$A = a.b.c.$$

If the atom has a quadratic base, $a=b$, we have

$$A = a^2.c.$$

If provided with one or several pyramidal additions, we have

$$A = a.b.c + k.$$

Elements will of course show similar properties when of similar form (a theorem which I have demonstrated in my notes many years ago—and which is closely allied to the well known properties of isomorphous bodies). Thus prismatic atoms will, when they have the same base $a.b$ and only differ in length $c=n$, form a natural group

$$A = n \times ab,$$

or when quadratic, $A = n \times a^2$,

or when with pyramidal additions,

$$A = k + n.ab,$$

or $A = k + n.a^2$, etc.

We shall here refer only to those natural groups of elements that have been treated of in the preceding, viz: the chlorine group, the oxygen group, the group of the alkaline earths and the group of the alkalies.

Oxygen group; quadratic. Formula $A = n.4^2$.

	n	A	Calc.	Obs.	Error.
Oxygen,	1	$1\cdot4^2 =$	16	16	0·0
Sulphur,	2	$2\cdot4^2 =$	32	32	0·0
Selenium,	5	$5\cdot4^2 =$	80	80	0·0
Tellurium,	8	$8\cdot4^2 =$	128	128	0·0

Chlorine group; quadratic. Formula $A = n.3^2 \pm 1$.

	n	A	Calc.	Obs.	Error.
Fluorine,	2	$2\cdot3^2 + 1 =$	19	19	0·0
Chlorine,	4	$4\cdot3^2 - 1 =$	35	35·5	+·5
Bromine,	9	$9\cdot3^2 - 1 =$	80	80	0·0
Iodine,	14	$14\cdot3^2 + 1 =$	127	127	0·0

Alkaline group; quadratic with pyramid. $A = 7 + n.4^2$.

	n	A	Calc.	Obs.	Error.
Lithium,	0	7		7	0·0
Sodium,	1	$7 + 1.4^2 =$	23	23	0·0
Potassium,	2	$7 + 2.4^2 =$	39	39	0·0
Rubidium,	5	$7 + 5.4^2 =$	87	85·4	−1·6
Cæsium,	8	$7 + 8.4^2 =$	135	133·0	−2·0

Alkaline-earths group; quadratic. $A = n.2^2$.

	n	A	Calc.	Obs.	Error.
Magnesium,	3	$3\cdot2^2 =$	12	12	0·0
Calcium,	5	$5\cdot2^2 =$	20	20	0·0
Strontium,	11	$11\cdot2^2 =$	44	43·8	−·2
Barium,	17	$17\cdot2^2 =$	68	68·5	+·5

We cannot here go into any detail as to the relation of these formulæ to the numerical relations discovered by Carey Lea, Dumas and others; we hope soon to be enabled to publish our labors on the constitution of the elements. Neither can we here discuss these formulæ in the sense of the mechanics of atoms, deducing the physical and chemical properties of the elements from these formulæ; these interesting relations also we must delay till some future, but I hope not a very distant, time. Nor is this the place to discuss the few slight deviations noticed. Our aim here is to make use of these our old formulæ in spectral analysis.

First, then, we notice that the alkaline-earth metals are quadratic, so that their spectra are the result of two systems only of lines. Further, having a common base (2^2), they must show one set of differences, either absolutely or nearly equal—which, until we have an analytical investigation hereof, or fuller experimental results, we cannot decide. But this is precisely what has been found in § 22, where the intervals were found respectively to be

Magnesium	0·00000157 mm.
Calcium.....	160
Strontium	161
Barium	163
Mean,	160

We might now discuss the occurrence of the *dimension-figures* of the atoms in the corresponding spectra as *intervals;* such for magnesium 2 and 3, for calcium 2 and 5, etc. Such coincidences are pretty numerous, but a fuller stock of still more reliable measurements will be required for the metallic spectra.

For the *chlorine group* we have the following dimension n, and distance d of lines, according to the given tables.

	n	d	$n.d$
Chlorine,	4	47	188
Bromine,	9	25	225
Iodine,	14	8	112

Then by doubling the interval 8 for iodine we get $14\times16=224$; or *the distance of the lines is nearly inversely proportional to the atomic dimension n.*

The other dimension of this group is 3, and is fairly represented in the intervals. Thus we have in the spectrum of *iodine*, neglecting the two extremes which are not necessarily complete, the intervals (see § 7)

$$21=3\times7$$
$$63=3\times21=3\times3\times7$$
$$23+4=27=3\times9\ =3\times3\times3$$

where the dimensions 3 of the base 3^2 is fully represented. The extreme intervals are $77=3\times26$ less 1, and $29=3\times10$ less 1.

Bromine gives the intervals (§ 6)

$$15=3\times5$$
$$1=1$$
$$3=3\times1$$

or again the dimension 3.

Chlorine has the intervals (§ 5)

$$9=3\times3$$

and $$5=3\times2-1$$

which, as 5 is probably not complete as far as it goes, again pronounces the dimension 3.

Thus we see in the spectra of the chlorine group a full and accurate representation of the atomic dimensions of these elements as expressed in their formula $A=n.3^2\pm1$, where $n=4, 9, 14$. I regret that I have no measurements for the *fluorine* spectrum.

§ 31. *Conclusion.*—In the preceding we have found, by means of the most accurate determinations of Ditscheiner and Plücker, that for the thirteen elements considered (viz., hydrogen, oxygen, nitrogen, chlorine, bromine, iodine, mercury, sodium, magnesium, calcium, strontium, barium, iron, and besides four compounds): *the dark lines of the elements are equidistant throughout the spectrum, but of varying intensity, many not being observed (or observable) at all; the intervals between the observable lines are expressible as simple multiples of the equal distance indicated by all.* It may be that the lines are a little farther apart in the more refracted blue part of the spectrum; see § 28.

We have, further, by considering the spectra of seven elements, viz., magnesium, calcium, strontium, barium and chlorine, bromine, iodine, found that *the dark lines of the elements are related to the atomic dimensions, considering the elements composed of one single primary element, "Urstoff."*

Thus we found the four alkaline-earth metals, having the same base, 2^2, to give almost identically the same principal distance of the lines; the mean was $0^{mm}\cdot00000160$ in wave-length. Also in the chlorine group most remarkable confirmations of this law were discovered.

It is now about twelve years since we first started the hypothesis of one primary matter as the element of elements, not in the shape of a philosophical idea, but as a physical hypothesis, *making it the basis of a theoretical, mechanical deduction of the properties of the elements.* Our first communications met with no favor: nevertheless we have continued to develop the consequences of this hypothesis.

It seems as if spectral analysis has shaken the axiom of the elementary nature of the so-called chemical elements in minds formerly adverse to questioning that axiom. Believing the sci-

entific public now more apt to give a hearing to our theory, we intend to publish a series of articles, giving the properties of the chemical elements as functions of their atomic weight, this expressed as in the few instances given in § 30. We hope to prove, that *the unity of matter is as real as the unity of force*—both being the creative work of *one* all-pervading being.

Iowa City, Iowa, July, 1866.

ART. XLIX.—*Contribution to the Chemistry of the Mineral Springs of Onondaga, New York;* by CHARLES A. GOESSMANN, Ph.D., Chemist to the Salt Company of Onondaga.

[Concluded from page 218.]

III. *How does carbonate of lime act upon a solution of chlorid of magnesium?*—I boiled for sixteen hours two grams of finely pulverized carbonate of lime with from forty to fifty cubic centimeters of a solution of caustic magnesia, in hydrochloric acid, containing 1·336 grams of chlorid of magnesium; the latter solution had, for obvious reasons, been heated with a small excess of magnesia, and was consequently of slight alkaline reaction; the water which evaporated during boiling was from time to time renewed. The hot filtrate at the close of the treatment contained 0·2122 grams of lime; while the residue left upon the filter contained merely traces of magnesia. In repeating this experiment, but with the difference that I evaporated directly to dryness (at from 190° to 210° F.) and subsequently extracted the residue by means of 50 cubic centimeters of distilled water, I found that the solution thus resulting contained but 0·0794 grams of lime. The indifference of carbonate of lime to chlorid of magnesium at common temperatures, is a fact stated long ago by Karsten. The peculiar readiness with which the chlorid of magnesium parts, in a diluted solution and at a higher temperature, with some of its chlorine, in the form of hydrochloric acid, most likely causes in this instance the formation of some chlorid of calcium; while the freed magnesia enters into combination with the main bulk of its chlorid, forming a soluble oxychlorid. The duration of treatment and the temperature influence, to some extent, the degree of change, which in itself is very limited, and probably only applicable to higher temperatures.

IV. *How does carbonate of magnesia act upon chlorid of calcium?*—A solution of 0·7660 grams of chlorid of calcium in 50 cub. cent. of water was boiled for nearly an hour with an excess of carbonate of magnesia, equal to 1·0264 oxyd of magnesium when

an examination proved that only 0·0246 grams of oxyd of calcium, equal to 0·0470 chlorid of calcium were left in the solution; while the missing lime had been replaced by magnesia: or, in other words, the chlorid of calcium, originally in solution, had been replaced by chlorid of magnesium. The excess of carbonate of magnesia employed for the operation, as left upon the filter, contained 0·6480 grams carbonate of lime (=0·7193 chlorid of calcium). The mutual decomposition is therefore, under proper circumstances, quite rapid and complete. There was no reason to apprehend any material difference in the results if chlorid of sodium should have been added. To prove this by experiment, some preliminary test of the solubility of carbonate of magnesia in a solution of chemically pure chlorid of sodium was required. I accordingly prepared such a solution of the strength of the Onondaga brines =16·5 per cent of chlorid of sodium, digested it for twenty-four hours with an excess of a well-washed commercial, as well as a freshly prepared, carbonate of magnesia, and obtained from the solution in both cases corresponding results—in the first case, 0·0297 per cent of magnesia, and in the second, 0·0265. Becoming thus informed of the solubility of carbonate of magnesia in a brine of the strength mentioned, I repeated the experiment, substituting Onondaga brine for a simple solution of chlorid of sodium. The brine already contained in solution (in the form of chlorid of magnesium) 0·0575 per cent of magnesia. An examination of the filtrate proved that the solution now contained 0·1324 per cent of magnesia. The whole amount of chlorid of calcium which the brine originally contained, and which was equal to 0·1400 per cent of chlorid of calcium, had been changed into a corresponding amount of chlorid of magnesium. Temperature and concentration do not, it seems, materially affect the action of carbonate of magnesia; while the solubility of the carbonate of lime formed is somewhat governed by both those conditions.

These few experiments are valuable in two directions. They explain to some extent, the reason for the preference here claimed among the various arrangements of the analytical results in the combinations presented below; and they indicate in what particular manner changes will take place when spring waters and brines become mixed. The mutual action of these two solutions upon each other in regard to changes in their chemical composition as pointed out, is, in my opinion, certain; and its magnitude is in a less degree a matter of time than of the quantities in which they happen to mix. As carbonic acid gas increases the solubility of a number of compounds here under consideration, its presence either as gas, or in the form of a bicarbonate, must, for that very reason, be highly favorable for bringing about some of the changes above described.

In the following final arrangement of my quantitative analytical results (see page 216), I favor No. I. in each case as the form best illustrative of the teachings of my investigations; for I am inclined to believe that the least objectionable basis for arranging analytical results, is to state them as they are obtained from an analysis of the solid residue left after a careful evaporation to dryness at common temperatures. In taking this view I am fully aware of the various changes of affinities which the alterations in temperature and concentration, in many instances, exert. Nos. II. and III, in each corresponding case, represent the same analysis in forms based upon views differing from those adopted in this paper.

Arrangement of the above various analytical results according to three different views.

I.

	a. (Willow st.)	*b.*(Prospect Hill.)	*c.*	*d.*	*e.*(Syracuse brine.)
Sulphate of lime,	0·3817	1·5214	0·6625	0·60250	5·7720
" magnesia,	0·1582	0·1764	....	....	....
" soda,	0·0311	0·0258	0·4508	0·22560	....
Carbonate of lime,	0·2946	0·2024	0·3942	0·26130	....
" prot. iron,	....	....	not det.	....	0·0440
Chlorid of sodium,	....	....	11·0844	10·02313	155·3170
" potassium,	....	....	....	....	0·1090
" magnesium,	0·0209	0·0169	0·3016	0·30340	1·4440
" calcium,	....	....	....	....	1·5330
Bromid of magnesium,	....	....	....	0·00270	0·0240
Silica,	0·0050	0·0035	0·0049	0·01770[a]	..
Free carbonic acid,	not det.	not det.	not det.	not det.	not det.
&c.					water 885·7570

II.

	a	*b.*	*c.*	*d.*
Sulphate of lime,	0·3817	1·5214	0·6625	0·60260
" magnesia,	0·1582	0·1977	0·3810	0·19065
Carbonate of lime,	0·2946	0·2024	0·3945	0·26130
Chlorid of sodium,	0·0257	0·0208	11·4572	10·20893
" magnesium,	....	....	....	0·15247
Bromid, "	....	....	....	0·00270
Silica,	0·0050	0·0045	0·0049	0·01770[a]
Free carbonic acid,	not det.	not det.	not det.[b]	not det.

III.

	a.	*b.*	*c.*	*d*
Sulphate of lime,	0·5851	1·7452	1·0943	0·81830
Carbonate of lime,	0·1451	0·0334	0·0769	0·10280
" magnesia,	0·1256	0·1384	0·2667	0·26820
Chlorid of sodium,	0·0257	0·0208	11·4572	10·39680
Bromid of magnesium,	....	....	....	0·00270
Silica,	0·0050	0·0045	0·0049	0·01770[a]
Free carbonic acid,	not det.	not det.	not det.[b]	not det.

a With some alumina. *b* Also some protox. iron and bromine undetermined.

The changes going on in this class of mineral waters as long as free carbonic acid or bicarbonates are present, as well as the

consequences resulting from the application of higher temperatures for concentration, have, no doubt, been instrumental in causing views like those illustrated in No. 2 of each analysis. To represent the strongest acid in combination with the strongest base, as shown in No 3 of each analysis, is but slightly supported if the cases presented are subjected to a close investigation.

Glancing over the various analyses, *No.* 1 *in every instance*—disregarding for the present their differences in the relative proportions of the same compounds—we find it worthy of particular notice that the waters *a* and *b* contain chlorid of magnesium,[1] sulphate of soda, sulphate of magnesia and carbonate of lime; *c* and *d* contain the same compounds (except sulphate of magnesia) besides a considerable quantity of chlorid of sodium. The *brine proper* contains no sulphates except sulphate of lime, common to all; while of the chlorids, besides chlorid of sodium and chlorid of magnesium, *chlorid of calcium*, (instead of the carbonate of lime) is present. From what has been stated it appears that, so far as *a*, *b*, *c* and *d* are concerned, a more or less proportionate access of carbonate of magnesia, in proper form,[2] to a solution of chlorid of sodium and gypsum in the presence of carbonic acid, can explain their differences of composition. The brine, even if exposed to the influence of a proportionate access of carbonate of magnesia in a suitable condition, will ultimately change its composition in such a manner as to resemble most closely that of the different waters, provided the excess of chlorid of sodium is left out of consideration.

A short recapitulation of what has been adduced in these pages may demonstrate this last assertion. Carbonate of magnesia and gypsum form, in the presence of carbonic acid, sulphate of magnesia, and carbonate of lime. Carbonate of magnesia, gypsum and carbonic acid, in the presence of chlorid of sodium, arrange themselves, at a common temperature, according to their relative proportions;—in cases where the chlorid of sodium is either equivalent to, or exceeds the gypsum and the carbonate of magnesia—sulphate of soda, chlorid of magnesium and carbonate of lime will be produced, (Anthon), while under circum-

[1] The brines of Onondaga contain traces of iodine; and so do the mineral waters, *c* and *d*, which are liable to a direct access of brine. I have omitted to place this fact more prominently on record, for the percentage is very small, though bromine exceeds the iodine; neither, however, have any immediate bearing on the various questions proposed. I have noticed that inferior pickle (mother-liquor), as sometimes found in the vats employed in the manufacture of salt by solar heat, shows frequently, during the summer season, decided indications of free iodine, and particularly of bromine. A peculiar condition of the atmosphere (ozone?) seems to cause their disengagement. A similar reaction was noticed during a thunderstorm within the past summer season; a discharge of lightning passed from a telegraph pole (which it had struck) into a tank containing stored brine.

[2] Compare for further illustration, Dr. Perry's lectures, Chemical News, London, 1864, No. 218, etc. Also T. Sterry Hunt's publications before mentioned.

stances where the gypsum and carbonate of magnesia exceed the chlorid of sodium, or under the influence of a certain higher degree of temperature, the product will be sulphate of soda, chlorid of magnesium, carbonate of lime, and sulphate of magnesia. The *essential difference between the brine and the spring waters consists, as has been noticed, in the fact that the former contains chlorid of calcium instead of carbonate of lime contained in the latter.* The presence of chlorid of calcium in the brines practically excludes all sulphates, except sulphate of lime. A sufficient amount of carbonate of magnesia, added to the Onondaga brine, displaced quite readily the chlorid of calcium, by forming chlorid of magnesium and carbonate of lime; and would have displaced, finally, at the expense of the gypsum produced (if exceeding the chlorid of calcium in amount), sulphate of soda and chlorid of magnesium, provided an excess of free carbonic acid were secured during the whole action. Free carbonic acid never fails to be present in the cases presented. The changes which must occur when the brine and spring waters become mixed are, in view of the preceding statements, quite obvious. The sulphate of magnesia and sulphate of soda of the waters act upon the chlorid of calcium of the brine, producing sulphate of lime, and the chlorids of magnesium and sodium; while the carbonate of lime contained in the spring waters enter simply into the mixture. The observation, that in several instances carbonate of lime has been found covering the crystals of gypsum separated in the wooden vats during the concentration of the brine by solar heat, may find its proper explanation in the temporary existence of circumstances where an access of spring water to the brine has happened.

Looking at these facts in regard to the brines from a mere practical point of view, we must admit that an admixture of the waters of the surface springs is not likely to alter seriously the brines for any length of time; for its access, even on a small scale, will readily be observed in consequence of a rapid decrease in the strength of the brine. Economical considerations will, no doubt, alone suffice to urge, in time, efficient provisions to prevent such a serious depreciation of the commercial value of the brine. The salt manufactured from such a brine suffers no deterioration in properties, for the carbonate of lime imparted will be removed during the preliminary treatment of purification of the brine in the process of salt manufacture; and the additional amount of chlorid of magnesium, being in the cases contemplated mainly formed at the expense of chlorid of calcium, is thus of too trifling consequence to require any serious notice. In regard to the water of the springs, the result of a union with brine must prove quite different, if merely on account of the differences in concentration—a fact most unmistakably demonstrated by the analytical results obtained from the water of the springs *c* and *d*.

In adopting these views I have already entered, as may have been noticed, upon the discussion of the third question proposed on page 212. The specific properties of springs are due, by general consent, to the peculiar geological features and physical conditions of the locality where they originate, and which they traverse in their course to the surface. The brines and waters here in question will bear, no doubt, such marks. I am inclined to consider sample *a* a true representative of our surface formations, rich in partially disintegrated magnesian limestone, containing pebbles, etc. Samples *b*, *c* and *d* have undoubtedly suffered by contact with the shales of the Onondaga Salt Group, (gypseous). Samples *c* and *d* have been exposed, in addition, to an access of brine. The brine, most likely, originates at a lower depth, or a more remote point; its contact, if any, with a limestone formation containing magnesia, was either under disadvantageous circumstances or the formation was comparatively deficient in available carbonate of magnesia;[1] and its access to the waters *c* and *d*, may happen through fissures from below; or may be due to a surface percolation, or, in some instances, to both causes.

Proper, detailed records of the exact geological features of the localities here under consideration are quite deficient, as I have before mentioned; and examinations beyond the search for paying wells have not been made of late. Certainly no reliable records concerning the real conditions of the strata which directly underlie our area of red shale—if bearing salt water, etc., have come to my knowledge. The importance of such information, it must be admitted, cannot be overrated when engaged in tracing the origin of our brines. All that is at present known of the Onondaga brines and their sources may be summed up in the following statements:

1. The depressions in the Onondaga Shales are filled, in some localities, to an extent of nearly three to four hundred feet in depth with a diluvial deposit (detritus), varying from the coarsest gravel to the finest drift sand.[2]

2. The layers of coarse gravel and fine sand alternate without any distinct order or extent.

3. The gravel has frequently been formed into a conglomerate of great hardness, commonly called hard-pan—an impermeable layer which intersects, more or less efficiently, the various strata of the loose material.

4. A formation of more recent origin, consisting of a red loam

[1] Dolomite and limestone of a dolomitic character—are slowly acted upon; Karsten, Haidinger, Hunt.

[2] The alternation of impermeable and loose strata may have some bearing upon the artesian character of the salt springs of Onondaga.

or loamy sand, covers, frequently, to a considerable extent, and to a depth of from 30 to 40 feet, the lower diluvial deposits.

5. The Onondaga red shale has been struck everywere, when the boring was continued beneath the brine-bearing drift masses; near the eastern embankment at from 90 to 180 feet;—toward the middle of the valley, between Salina and Geddes, at about 382 feet.

6. The brine proper makes its appearance at about 100 feet below the level of the surface.

7. The brine rises by means of boring and tubing to the level of the lake surface or within from 10 to 15 feet of it; the degree of its rise depends apparently on the specific gravity of the brine. The most concentrated brine remains lowest.

8. The yield of a well, independently of the concentration of the brine, depends on the size of the gravel or sand around the lower termination of the tubing.

9. The brine does not increase or decrease in strength during the winter season, when no pumping takes place; its temperature is from 52° to 53° F.

10. The deep wells bear the heavy drafts of brine during the summer season without suffering in strength, while the shallow wells decline.[1]

11. The lowest depth does not, in all cases, guarantee the most concentrated brine.

12. The red shale apparently bears irregularities of stratification independent of the peculiar form of a basin.

13. The outcrop of the red shale on the eastern embankment of the lowlands—at Green Point—contains veins of gypsum interspersed with specular iron ore.

The occurrence of the peroxyd of iron as a pseudomorph of chlorid of iron in this local outcrop of the red shale is deserving of some attention. The peculiar manner in which the gypsum and that ore present themselves, sometimes in veins along side of each other, sometimes the latter surrounded and enclosed by the former, (besides the resemblance between some of the adjoining gypsum to the hardened masses of gypsum separated from its boiling solution in salt water,) are facts which seem to point to the existence of some peculiar local disturbance due to subterranean heat. The presence of serpentine at James street height, and the elevation of the localities where casts of chlorid of sodium have thus far been found, may also bear some relation to

[1] The strength of the brine from the various wells varies from 45° to 76°, Salometer at 60°. The weaker brine is frequently found near the outskirts of the lake basin, (lowlands), and in the shallow wells. The strongest brine has thus far been obtained mainly from wells near the beach of the lake, and along the banks of Onondaga creek, (toward the center of the valley), between Geddes and Salina. The annual production of salt for the past ten years averages about seven million of bushels of 56 lbs.

the presumed changes which some of the surrounding localities have suffered in regard to their original stratification.

To what immediate causes such local disturbance may be attributed, at what period it happened, to what extent, and with what consequences it may have manifested itself, must, for the present, remain a matter of conjecture. It is, however, gratifying to know that measures are under contemplation in proper quarters, which, it is to be hoped, may result in advancing our information relative to the highly important question: *What and where is the real source of the valuable brines of Onondaga?*

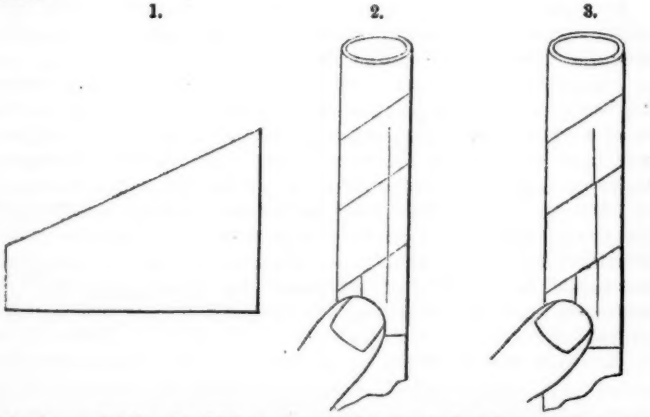
Syracuse, Feb., 1866.

ART. L.—*On some new Manipulations;* by M. CAREY LEA.

I. GRADUATION OF BURETTES.

(1.) *Selection of Tube.*

ALTHOUGH no tube is sufficiently regular to dispense with subsequent exact calibration with a good balance, yet it is very desirable to have as near approach as possible to perfect uniformity, both in order to diminish the trouble of the weighings, and to a certain extent, that of the calculations, in actual use. In the following manner a variation in the diameter of the tube under examination, amounting to $\frac{1}{1200}$ or less can easily be detected.



A piece of thin glazed letter paper is cut to the shape represented in fig. 1, this is wound tightly round the tube, making the successive folds perfectly correspond at bottom, a card is held to the paper, and a thin line is drawn with a fine pointed pencil, passing over three folds of the paper as represented in fig. 2.

The paper is then loosened, slipped a few inches along the tube and tightened again, keeping the lower edge exactly even. A very small difference in the diameter of the tube will cause the line to appear broken, as represented in fig. 3, instead of straight. A displacement from end to end amounting to one $\frac{1}{100}$ of an inch is easily observable, and as this difference corresponds to a little over six diameters, it is clear that a difference in the mean diameter of $\frac{1}{600}$ of an inch or less, makes itself evident to the unassisted eye. With an ordinary lens, a difference of $\frac{1}{200}$ of an inch is easily distinguishable, corresponding to a difference of diameter of $\frac{1}{200}$ of an inch. When great accuracy is desired it is advisable to stretch the paper band strongly beforehand, whereby it acquires a very slight permanent increase of length, and is not further extended by the very moderate force requisite to wrap it tightly round the tube.

It is so easy in this manner to detect slight differences that I have never been able to find a piece of tube of a foot or more in length which would bear this test when applied with the utmost rigor. But by examining large quantities of tube, pieces can be found sufficiently good for graduation. These are to be cut out from the tube in which they are found, and the part which has been ascertained to be sufficiently regular, must be carefully marked that the graduation may not be carried beyond that portion.

(2.) *Graduation of the Burette.*

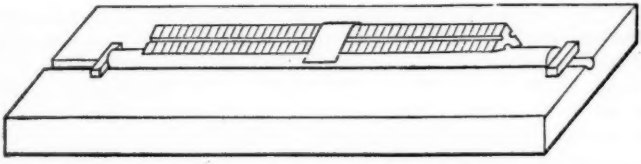
All modes of graduation are necessarily the same in principle, and consist in transferring divisions from a scale to the tube. Accurate three-edged engineer's scales are easily to be had, and are very convenient for this purpose. The glass tube is secured by cleats into a groove in a block of wood. And here I may remark that the directions usually given in respect to the groove in which the tube rests, are certainly mistaken in recommending the groove to be part of a circle of a diameter *greater* than that of the tube. Such an arrangement causes the tube to rest loosely on the bottom of the groove, and to rock easily from side to side, touching as it does, in one place only. But if the groove is that of a circle of diameter rather *less* than that of the tube, the latter rests on the edges of the groove, and remains steadily in its position.

Before fastening the cleats, and while the tube is lying in the groove, touching its edges, a diamond pencil is drawn along the tube, the edge of the groove serving as a rule, and a line is made from one end of the space intended to be graduated to the other. The tube is then rotated on its axes a quarter of an inch, and another line is drawn parallel with the former. These lines serve

to limit the length of the single degrees, each fifth and tenth being drawn beyond them.

The cleats are then fastened, the three-edged rule is laid with one of its edges resting on the tube, and its ends also are secured by cleats (these last are not represented in the figure). A piece

4.



of thin sheet brass (such as is used for enveloping combustion tubes) is bent at a sharp angle, and its right hand edges cut to an exact right angle with the bend. This is rested on the upper edge of the ruler, extending across its front face and over the tube—its right hand edge serves to guide the diamond pencil in transferring the divisions. Twentieths of an inch, or millimeters are easily and exactly ruled in this way, and the burette is ready for calibration.

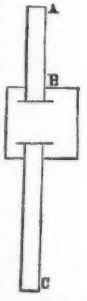
(3.) *Calibration.*

When a burette contains a portion of liquid, it is a matter of great nicety to determine the division or fraction of a division to which the surface of the liquid corresponds. To save the labor of holding the paper with the lower half blackened behind the tube, it is convenient to substitute card board, and to cut two parallel slits in it so as to form a band, which being slipped over the burette, the card maintains its own position. The use of paper blackened on the lower half, gives, according to Mohr, all the accuracy desirable. Bunsen, on the contrary, uses a cathetometer. That the former method is insufficient any one may satisfy himself by placing the card in position, and then moving his head slightly in a vertical direction when the black line which is assumed to mark the surface of the liquid, will be found to move also, and though the change is but small, it will be found that even the slightest movement of the eye produces a change in the position of the line which gives a difference in results easily detected by a good balance. Let us suppose that the observer notes the position of the black line before and after removing a portion of the liquid, he cannot be certain that his eye has occupied the same relative position in both cases, unless he takes due precautions. Every observer does not possess a cathetometer, and the following arrangement which is of extreme simplicity, will be found to answer every purpose.

A slip of wood is provided $\frac{1}{10}$ inch thick, $1\frac{1}{2}$ inches wide, and 2 feet long. A piece of card about 4 inches square has two parallel slits made in it at one end, one slit half an inch from the top, the other the same distance from the bottom. These slits are of such a length as just to permit the piece of wood to be passed through them, presenting the appearance represented in the margin.

To make a reading, the half black card previously spoken of is slipped over the burette, and the line of separation between the white and black is brought one millimeter below the black line which marks the surface of the liquid. The instrument represented in the margin, is then placed beside the burette, the lower end resting on the table, and the card is raised or lowered until the edge B exactly corresponds with the line of separation in the black and white card. The stick is then drawn toward the observer, the end B still resting on the table, and the observer places his eye so as to keep the line B in range with the line of separation, and makes the reading.

5.



Another precaution which I regard as essential, and which I have nowhere seen mentioned, is the following: The observer should place himself where he has a strong *side light*. The half black half white card must not be placed parallel to the eyes of the observer, but must be turned toward the light, so as to make an angle of 45° with the line of vision. In this way a strong light is reflected from the card and thrown through the burette in a manner greatly conducive to clearness of vision, and the black line which marks the surface acquires a peculiar sharpness.

I cannot better illustrate the necessity of these precautions, (especially the use of the little instrument above described, and which may be termed the *eye adjuster*) than by the comparison of the following results obtained without them and with them.

Distilled water corresponding to the divisions of a tube prepared in the manner above described was carefully weighed in an accurate balance.

1. *Without the precautions.*

Mean of first three trials,	2·023	grams.
" " second " "	1·974	"

2. *With the precautions.*

Mean of first three trials,	2·046	grams.
" " second " "	2·047	"
" " two more "	2·050	"

It will be seen that the trials without the precautions which I recommended gave results which were not only discordant with each other, but were *all wrong* and below the truth. With the

precautions, on the contrary, the mean of the first three differed from that of the second three by only a single milligram, or less than the sixtieth part of a drop.

To show that the latter results were of no fortuitous exactitude, I may mention other instances. The distilled water corresponding with a given space was weighed *six* times and the mean found was 2·090 grams. Subsequently it was deemed advisable to submit this to a second verification, and the mean of *nine* trials gave 2·091. To the above two cases I may add the following:

Table No.	3,	mean of	first	4	weighings,	2·054,	of	4	more,	2·0515
"	"	4,	"	"	"	3	"	2·387,	"	4	"	2·362
"	"	5,	"	"	"	3	"	2·365,	"	3	"	2·370
"	"	6,	"	"	"	2	"	3·355,	"	2	"	3·551
"	"	7,	"	"	"	2	"	3·577,	"	2	"	3·577
"	"	8,	"	"	"	2	"	2·3605,	"	2	"	2·3630

Comparing the first and second columns, that is, the first set of determinations, with the second we find that the maximum error was ·005, and the minimum ·0, or exact correspondence. The average of all the cases was 3·3 milligrams, or about one-twentieth of a drop. It would be difficult in burette analysis to obtain greater accuracy than this, for if we are using, for example, ten per cent solutions, the maximum error would reach half a milligram of the reagent used, and the average error would be one-third of a milligram.

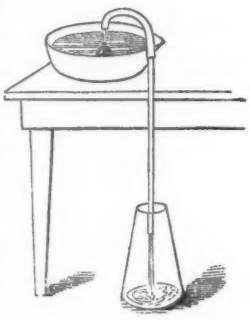
I might greatly extend this table of verifications, but I think what I have cited will be sufficient to show that the very simple precautions which I here propose, viz., the position of the burette and card relatively to the light, and still more, the use of an eye-adjuster consisting of a slip of wood and sliding card are sufficient to carry us to the extreme limits of accuracy which the burette is capable of affording. It also shows that the burette is entitled to great confidence where carefully used and when the reactions which mark the termination of the operation are perfectly distinct and sharp.

II. Inverse Filtration.

Large quantities of mixed solid and liquid matter may be filtered with a funnel of the smallest dimensions very speedily and conveniently, by proceeding in the following manner. A piece of stout muslin is strained over the mouth of a small funnel, and tied securely at the neck, taking care that the string covers all the folds perfectly. A piece of india-rubber tube is then passed over the open end of the stem of the funnel: this piece may either be several feet in length, or it may be shorter, and the difference be made up by inserting a glass tube. The funnel and tube are then filled with water, the open end of the

tube is closed with the finger and the funnel is quickly inverted in the vessel containing the mixture to be filtered. The other end of the tube hangs down into a convenient vessel placed on the floor. If it is desirable that water shall not be added to the mixture, the funnel is inverted empty, and the air is drawn out by a pipette inserted into the open end of the india rubber tube.

6.



It is evident that this arrangement is a combination of filter and siphon, and the pressure of the column of water in the longer leg of the siphon expedites the operation very much and leaves the solid portions much drier than ordinary. As the liquid begins to be exhausted, the solid portions are to be gathered round the funnel with a spatula. When the filtrate ceases to run, the funnel is left full:—in order that this may not return upon the solid matter, the funnel is lifted out of the vessel and the broad end quickly turned uppermost, when the contents of the funnel flow down the tube.

When a precipitate is to be well washed this mode is evidently not applicable, although a tolerable washing is perfectly practicable. But when liquid and solid matters are to be quickly separated on a large scale, it is very useful. When masses of small crystals strongly retaining the mother water, are to be freed from it, this may be done more quickly and more thoroughly than by ordinary filtration. Many other cases will readily suggest themselves. For example, when potash has been boiled with lime to render it caustic in large vessels, it is usually drawn off with a siphon into bottles and left twelve to twenty-four hours to settle, and then must be carefully decanted. It is far less trouble to filter it in the above manner in the act of removing it by a siphon. And so with many of the rough operations which occasionally present themselves in the laboratory, and which are upon a scale rather exceeding the capacities of ordinary filtering funnels.

In the separation of crystals from the mother water, muslin will generally give a clear filtrate. In other cases it is necessary to place a piece of filtering paper inside the muslin. The paper must of course be of size sufficient to be secured by the twine at the neck of the funnel. But the force with which the column of water acts upon the muslin over the mouth of the funnel, draws it to a concave shape, and sometimes breaks the paper. To avoid this, the paper may be folded so as to fit the *inside* of the funnel, turning the edges over and securing them as before de-

scribed. To do this requires a little dexterity; the diameter of the paper must be six or seven times that of the mouth of the funnel, it must be folded across in two rectangular directions in the ordinary way, then opened and reversed and from opposite points in the edges folded some distance past the middle. The funnel is then passed into it and the edges are passed round the neck of the funnel, the muslin is next placed over the paper and the whole is secured round the neck with cord.

ART. LI.—*Experiments on the Electro-motive Force and the Resistance of a Galvanic Circuit;* by HERMANN HAUG.

ENGAGED in investigations into the true numerical relations between the consumption of zinc in a galvanic battery, and the mechanical power which, by means of an electro-magnetic engine, may be derived from the galvanic current under varied conditions both in form and quantity, I began the work with studying the electro-motive force and the internal resistance of the battery, with the view of comparing the latter and the external resistances in conductors and helices. To determine the constants I used Ohm's method. A single galvanic cell was, by means of short, thick copper wires, connected with a tangent compass and a rheochord. The tangent compass was of Poggendorff's construction, the needle suspended by a hair, and modified according to Gaugain, thus securing the proportionality between the intensity and the tangent. The rheochord was of thin platinum wire, of Poggendorff's construction, modified by Dubois-Reymond. Both instruments were made by the best artists in Germany.

The law of the maximum of the effect of a galvanic battery requires both the internal and the external resistances to be the same. For many important reasons, which, however, have no direct connection with the object of this report, I thought it essential not to confine myself to the sequence of this law, but to vary the proportions between internal and external resistances of the electro-magnetic engine, as widely as circumstances would allow. I therefore considered it best to study the constants of the galvanic battery too, at once under similarly extreme conditions as regards the internal and external resistances. To do so I was further guided by circumstances and reflections which it may be well to state here with a few words.

From a careful study of the problem of practical application of the electro-magnetism as motive power, I have become convinced that it is merely a question of economy—economy in any material to be consumed, as well as economy in power to

be derived from it, and to be transformed for the object in view. I found it necessary to exercise this economy from the very outset, and further on, at each and every step of all the processes and operations involved in the development and transformation of the power. One of these steps was the production of the galvanic current within a full circuit, from the free electricities of the poles of the open battery. A given quantity of electricity, in a galvanic current, will always, generally viewed, produce the same amount of electric process. But the free electricities requiring time for motion and combination (to use some kind of expression for the details of the electric process), remain during this time, subject to the influences of induction and the molecular qualities of the conductors. There may, consequently, occur something like a diffusion of the electricities, before the electric process is completed, and it is most probable that the direction of the electric process therefore, will not remain one and the same for the whole quantity of electricities, but that one part of the electric process will be executed in directions varying from the main direction, thus diminishing the actual effect of the process; and that the amount of this part, respectively the amount actually disposable, will depend upon and vary with certain circumstances. Now, as the electro-magnetic effect of the galvanic current is plainly and invariably governed by the direction of the electric process, it becomes evident that the amount of electro-magnetism to be derived from a given quantity of electricities, and available for any practical purpose, depends entirely on that part of the electric process which, at last, is going on in the main direction, and which is only a certain percentage of the whole electric process.

These considerations compelled me to be watchful, and this the more as I met with but one remark (by Buff, if I remember rightly) directly pertaining to this question. He found the electro-motive force of the battery increasing with the decrease of the measured intensities of the current. This circumstance, if true, would justify my views of the matter, and naturally must affect the economical applications of the galvanic current. The counter current too, de la Rive supposed to exist in a battery, may be mentioned in this connection.

My experiments gave results which it may be well to record. Though I cannot claim any excessive accuracy for my figures, still I am confident of their general correctness, and hope that they may be of some theoretical importance. They had an unfavorable influence on the proposed investigations.

The battery was a Bunsen's, one cell being of Stoehrer's, the other of Deleuil's construction; the former using a hollow carbon cylinder, the latter a plate of true gas coke. As liquids were used diluted sulphuric acid (respectively an acidulated so-

lution of sulphate of zinc) and strong nitric acid, or a properly acidulated solution of bichromate of potash instead.

My method of experimenting was as follows. After preventing the conducting wires from affecting the tangent compass, and determining the intensity of the current within the shortest circuit possible, increasing length of platinum wire was, without opening the circuit, introduced into it. The reasons for not opening the circuit after each observation, were: 1, the time required for every observation after the circuit had been opened; 2, within the electro-magnetic machine, a similarly gradual change of resistance is effected, by means of the brake; 3, the rheochord is just designed, partly, to relieve of that tedious practice of opening the circuit every time a new resistance is introduced, and no objections have been raised, so far, against its proper use.

To calculate the constants of the battery I followed the common rule, combining the highest intensity within the shortest circuit, with each and every lower intensity. Let I_1 and I_2, be the intensities, E the electro-motive force, R the internal resistance, and P_2, P_3 the respective length of the platinum wire of the rheochord, and we have

$$R=\frac{P_2 I_2}{I_1-I_2};\quad E=RI_1.$$

Table I gives the results of one series of observations with a Bunsen battery. For this table, as well as for all the others, I have to state that the figures on the "rheochord" line are centimeters of platinum wire, unless otherwise mentioned. In case two tangents are given, the observation of the direct intensities has been made, first in the beginning and afterwards at the end of the experiments; and every lower intensity has been combined with a highest intensity proportionately increased, or decreased, as the case might be, from the first to the last direct intensity. The rest of the table will explain itself, or will be explained later.

The figures for the internal resistance, as well as those for the electro-motive force, exhibit an increase amounting to 100 per cent within the range of the experiments, with the decrease of the lower intensity which is to be combined with the direct intensity, or with the increase of the external resistance. I confess I was not prepared to meet any such fact, since good experimenters frequently used pretty different and pretty high intensities; at any rate, the fact, if true, seemed to have been very much underrated. Of course, the lack of proper knowledge in this regard, and the general belief in the correctness of the law of Ohm, and its experimental proofs by many of the ablest observers, led at first to a severe doubt as to the value of my own

observations compared with those of other experimenters. , On the other hand, the increase was, on the whole, rather great, though it becomes very small from observation with 60 centimeters of platinum wire. Now the first circumstance, the general great increase, may perhaps be accounted for by the influence of temperature upon the resistances of conductors. The second circumstance, the slight increase from the experiment with 60 centimeters of platinum wire, may explain the fact of the results of other experimenters varying less, if they kept their experiments within narrower limits, and this was usually the actual case. The errors of observation may hide the normal increase, and may have led to the trifling with the matter, as generally observed.

Examining more closely some reports of other experimenters I was indeed able to trace the general increase of the constants of a galvanic battery with the decrease of the observed intensities. Thus I. Müller records the following results of observations with six cells of Daniell's construction. The resistance is expressed in meters of copper wire.

No. of battery.	Internal resistances for Meters of copper wire introduced into the circuit. 5	10	40	70	100	Mean values of them.
1.	2·85	2·85	3·20	....	....	2·97
2.	3·41	3·35	3·55	....	....	3·44
3.	3·02	3·05	3·23	....	....	3·10
4.	3·19	3·19	3·55	....	....	3·25
5.	3·08	3·13	3·40	....	....	3·21
6.	3·68	3·64	3·57	....	....	3·63
						20·6
Bat. of 6 cells,	18·20	19·03	18·01	....	....	18·31
				18·56	18·38	18·47

I think nobody can overlook the general increase of the internal resistance of every cell, except No. 6, with the increase of the external resistance, or with the decrease of the observed intensity. The sum of the mean resistances of all the six cells is 20·6, while the battery of all the six cells combined actually did not have more than 18·31, as calculated from the corresponding first three observations with 5, respectively 10, and 40 meters of copper wire in the circuit. Thus the internal resistance again results smaller, while the intensity of the observed currents is greater. Of course, the experimenter demonstrates from these figures that they are equal, to his satisfaction. But whatever the reason of this real increase of the galvanic constants may be, it seems to be a very wrong practice to accept the mean value of any number of observations as a true result for the internal resistance of a galvanic battery.

To convince myself more fully of the fact in question, I undertook other series of experiments, the results of which are

given in tables II, III, IV, and V. Since, according to the formula $E=RI_1$, both the electro-motive force and the internal resistance maintain the same relation, I calculated for these, and all the following tables, only the internal resistance. Table III shows an increase from 2·98 to 5·52, not quite as much as table I; though the direct intensity is about the same, while the external resistance has been increased very much. Tables III and IV plainly demonstrate, as a general rule, that the increase of the internal resistance becomes less remarkable when the direct intensity of the battery is low. This rule, however, has its exceptions, and table V exhibits a very remarkable case. The direct intensity of this battery is less than in table II, still the increase of the internal resistance is much greater than in this table.

These results vexed me the more, since I undertook the experiments mainly for practical purposes. The experiments delayed my proposed investigation, and I could not anticipate any direct profit. I therefore concluded to give up, for the present, determining the constants of the battery, as they were not very constant; and to content myself with determining the resistances of the helices and conductors of the engine. It was with this object in view that I made the series of observations of table V, with 80 inches of thin copper wire in the circuit. Combining both the direct intensity (1·2712) and the intensity with 80 inches of copper wire in the circuit (·6964), with each and every of the other observations, I could calculate the resistance of the 80 inches of copper wire, expressed in centimeters of platinum wire. If C be the resistance of the copper wire, it follows from

$$I_1=\frac{E}{R},\quad I_2=\frac{E}{R+C},\quad I_3=\frac{E}{R+C+P}\quad \text{that}\quad C=\frac{P(I_1-I_2)I_3}{(I_2-I_3)I_1}.$$

As table V shows, the resistance of the copper wire as calculated in this manner, increases with the decrease of the observed intensities, just the same as the internal resistance did. And no limit being fixed, there could not be placed any reliability in any one of these calculated resistances, nor could I, with such a method, determine at all any resistance, under circumstances similar to those I proposed to try within the electro-magnetic machine. I now was compelled to investigate the matter at once.

As mentioned above, the reason for the increase of the internal resistance may be the relations existing between the intensity of the current, the heat developed by it within the circuit, and the influence of temperature upon the resistance of conductors. Indeed, the heating of the platinum wire makes our unit of resistance *greater* than the resistance of a unit of length of the wire at common temperature. And the liquids in the cup being

heated, thereby offer *less* resistance. The internal resistance being diminished, and measured with an increased unit, will, of course, appear *smaller* than at low intensities. But knowing this, everybody ought to foresee the general increase of the constants of the galvanic current with the decrease of its intensity, instead of neglecting this fact entirely, or underrating its importance, and treating any variation of results simply as errors of observation.

I suppose that this interpretation, offering itself at the first glance, of the apparent increase of the constants, will be the one generally preferred. It is sustained by the circumstance, that the resistance of the platinum wire at beginning of red heat, is said to be a little over two times greater than its resistance at common temperature, and this ratio of increase of resistance coincides indeed pretty near with the maximum ratio of increase as given in the tables; intermediate cases not being comparable for want of proper control of the temperature of the platinum wire. Though I was pretty sure that in the observations of tables I and III, eight, respectively ten, centimeters of platinum wire had been rather far from becoming red hot, since the experiments were made at lamp light, and the wire deeply shaded; still, any lack in ratio of increase, from the heating of the platinum wire, might perhaps have been fully made up by the auxiliary of the liquids becoming warmer at high intensities, and their resistance being diminished. Being far from rejecting this explanation, in the whole, it is however just to remark, that it is indeed open to objection on a general ground, since all the experiments, the ratio of increase of the resistance of conductors with increase of temperature, as commonly understood, is based upon, must have been influenced, and their results augmented, by any other reason of such increase of resistance, if there be any other reason at all.

But this interpretation of the fact in question is liable to more direct and more conclusive objections, and I could not feel satisfied with it at all. In the first place, there were the experiments of table V, which seemed directly to contradict any such explanation. The ratio of increase of the internal resistance, the copper wire included, from 8·86 to 15·71 is $= 1 : 1{\cdot}773$
The ratio of increase of resistance of the copper wire, calculated separately, from 4·0 to 7·1 is $= 1 : 1{\cdot}775$
The true internal resistance, resulting either by subtracting the resistances of the copper wire from the respective whole internal resistances, or by direct calculation after the formula $R=\frac{I^2 I_2 I_3}{I_1(I_2-I_3)}$, increases from 4·86 to 8·61, or in the ratio of $= 1 : 1{\cdot}772$

Thus the ratio of increase of resistance of the copper wire is the same as, or even a little greater than, that of the true internal re-

sistance, or the liquids. Now it is evident, from the fact of the copper wire being heated much less than the platinum, and increasing its resistance in a higher degree than the latter (E. Becquerel gives the relations between resistance and temperature, for platinum $=100+0{\cdot}1861$ t.° Cels., for copper $=100+0{\cdot}4097$ t.° Celsius), that the ratio of increase of resistance of the copper wire should appear to be remarkably smaller than that of the liquids which constitute the main part of the internal resistance, under equal circumstances. The explanation above referred to, so acceptable in case of the internal resistance of the liquids, seemed therefore to be a failure in case of the copper wire, since I presumed my experiments accurate enough at least to trace the difference, naturally to be expected, between the ratio of increase of resistance of liquids and that of copper.

But there is a point of far greater weight to be considered. The common rule for the calculation of the constants of a battery, by combining the highest direct intensity with some lower ones, or any other specific "method" certain experimenters prefer, is rather arbitrary, since Ohm's formula knows of no such restrictions, but permits to combine any two or more observations. And in thus combining any two observations I at first expected to get more reliable results, especially by throwing out those observations in which the platinum wire still had been heated very much. But these calculations seemed to run perfectly wild, giving much greater values than ever before. Combining, for instance, the intensity 0·115 with 100 centimeters of platinum wire, in table I, with each of the following three intensities, after the formula $R=\frac{P_2I_2-P_1I_1}{I_1-I_2}$, the values for the internal resistance became respectively 23·75, 31·67, 21·15, with a mean of 25·52, which, compared with the first value for the resistance (3·48), gave an increase from 1 : 7·33, thus far greater than anybody could attempt to explain by way of influence of temperature. I now became pretty much convinced that there exists indeed some other reason for the increase of resistance with decrease of intensity, and a reason very much more powerful than the influence of temperature, and one able to conceal, by way of its great ratio and the errors of observations, comparatively slight differences in the ratio of increase of resistance of liquids and copper as proceeding from the influence of temperature.

On the other hand, those great and much varying values seemed to pronounce my observations as perfectly worthless. I could not imagine that facts of such important bearing could have been overlooked. Though, it is true, the experimental results the best observers have arrived at do not harmonize with

each other to such a degree as to silence every doubt about their reliability.

However, my experiments were open to grave objections, as visibly indicated by the irregularity of the results. Indeed, the use of acids (nitric acid particularly) from former experiments may have caused some polarization to set in. The time required for a series of observations—the diminishing of the direct intensity usually occurring during this time—the change of the temperature and chemical composition of the liquids—the influence perhaps existing of the circuit being kept closed during the rapid but gradual change from one external resistance to another—all these circumstances may be considered susceptible of bringing on such extraordinary results as those just referred to.

TABLE I.

Bunsen's battery. Gas coke in nitric acid. The acids had been used previous to the experiments. The platinum wire, when shorter than 8 centimeters, became red hot.

Rheochord.	Compass.	Tangent.	Internal resist.	Electro-motive force.	Rheochord.	Compass.	Tangent.	Internal resist.	Electro-motive force.
0	60·8°	1·789	...	....	60	10·2°	·18	6·58	11·97
	61·7	1·857			80	8·	·14	6·66	12·16
8	28·5	·543	3·48	6·23	100	6·6	·115	6·69	12·27
10	26·2	·492	3·77	6·77	120	5·7	·099	6·84	12·6
20	19·25	·354	4·89	8·82	160	4·5	·079	7·11	13·15
40	13·0	·231	5·87	10·6	200	3·6	·063	6·97	13·05

TABLE II.

The same battery. The gas coke, after washing with water, had been exposed to the drying action of the air for 24 hours. The same acids.

Rheochord.	Compass.	Tangent.	Int. resist.	Rheochord.	Compass.	Tangent.	Int. resist.
0	55°	1·4281	....	100	5·1°	·0893	6·67
10	22·4	·4122	4·05	120	4·3	·0752	6·67
20	16	·2867	5·02	140	3·8	·0664	6·83
40	10·5	·1853	5·96	160	3·33	·0576	6·79
60	7·9	·1388	6·46	180	2·95	·0516	6·75
80	6·2	·1086	6·58	200	2·6	·0455	6·58

TABLE III.

The same battery, except the gas coke exchanged for another dry piece not used before. Acids the same.

Rheochord.	Compass.	Tangent.	Int. resist.	Rheochord.	Compass.	Tangent.	Int. resist.
0	61°	1·8040	....	180	2·8°	·0489	5·01
10	22·4	·4122	2·98	200	2·5	·0437	4·94
20	16	·2867	3·78	220	2·3	·0401	5·0
40	10·5	·1853	4·58	240	2·15	·0375	5·09
60	7·8	·1370	4·93	260	2·	·0349	5·13
80	6·15	·1077	5·08	280	1·9	·0332	5·25
100	4·95	·0867	5·05	300	1·85	·0323	5·47
120	4·25	·0744	5·16	320	1·8	·0314	5·67
140	3·65	·0638	5·13	340	1·65	·0288	5·52
160	3·2	·0559	5·12				

TABLE IV.—Bunsen's battery, with a common hollow cylinder. The acids had been used previously.

Rheochord.	Compass.	Tangent.	Int. resist.	Rheochord.	Compass.	Tangent.	Int. resist.
0	$\frac{15{\cdot}7°}{15{\cdot}4}$	·2811 ·2754	....	100	4·35	·0761	37·65
10	12·15	·2153	32·7	120	3·95	·0690	39·67
20	9·8	·1727	32·04	140	3·4	·0594	38·2
40	7·85	·1290	34·17	160	3·1	·0541	38·9
60	6·0	·1051	36·22	180	2·9	·0507	40·5
80	5·06	·0885	37·2	200	2·75	·0480	42·21

TABLE V.—Bunsen's battery, with gas coke. The acids had been used previously. The nitric acid, however, was mixed with sulphuric acid to restore the strength. The circuit contained 80 inches of thin copper wire.

Rheochord.	Compass.	Tangent.	Int. resist.	Resist. of 80 in. copper.	True inter. resist.	Rheochord.	Compass.	Tangent.	Int. resist.	Resist. of 80 in. copper.	True inter. resist.
0	$\frac{34{\cdot}5°}{35{\cdot}2}$	·6964	....	....	....	40	9·7	·1709	13·01	5·88	7·13
2	29·6	·5681	8·86	4·0	4·86	50	8·5	·1495	13·67	6·18	7·49
3	25·2	·4706	8·34	3·77	4·57	60	7·5	·1317	13·99	6·33	7·66
6	22·3	·4101	8·59	3·88	4·71	70	6·6	·1157	13·95	6·31	7·64
8	20·3	·3699	9·06	4·10	4·96	80	6·1	·1068	14·49	6·55	7·94
10	18·8	·3405	9·57	4·32	5·25	90	5·6	·0980	14·74	6·66	8·08
12	17·4	·3134	9·82	4·44	5·38	100	5·2	·0910	15·03	6·79	8·24
14	16·4	·2943	10·24	4·63	5·61	120	4·5	·0787	15·29	6·91	8·38
16	15·5	·2773	10·59	4·79	5·80	140	3·8	·0664	14·75	6·67	8·08
18	14·7	·2623	10·88	4·92	5·96	160	3·5	·0612	15·41	6·97	8·44
20	14·	·2493	11·15	5·04	6·11	180	3·2	·0559	15·71	7·10	8·61
30	11·4	·2016	12·22	5·53	6·69	200	2·9	·0507	15·71	7·10	8·61

The 80 inches of copper wire excluded, and the circuit closed directly, the current gave $\frac{51{\cdot}2°}{52{\cdot}4}$ at the compass, corresponding to a mean tangent of 1·2712.

(To be continued.)

ART. LII.—*On the Spectrum of a new Star in Corona Borealis*;[1] by WILLIAM HUGGINS, F.R.S., and W. A. MILLER, M.D., Treas. R.S.[2]

YESTERDAY, May the 16th, one of us received a note from Mr. John Birmingham of Tuam, stating that he had observed on the night of May 12 a new star in the constellation of Corona

[1] The Astronomer Royal wrote to one of us on the 18th, "Last night we got a meridian observation of it; on a rough reduction its elements are—

R. A. 1866, May 17, - - - $15^h\ 53^m\ 56^s{\cdot}08$
N. P. D., - - - - - $63°\ 41'\ 53''$

agreeing precisely with Argelander, No. 2765 of 'Bonner Sternverzeichniss,' declination +26°, magnitude 9·5." Mr. Baxendell writes on the 21st, "It is probable that this star will turn out to be a variable of long or irregular period, and it may be conveniently at once designated *T* Coronæ." Sir John Herschel informs one of us that on June 9, 1842, he saw a star of the sixth magnitude in Corona very nearly in the place of this strange star. As Sir John Herschel's position was laid down merely by naked eye allineations, the star seen by him may have been possibly a former temporary outburst of light in this remarkable object.

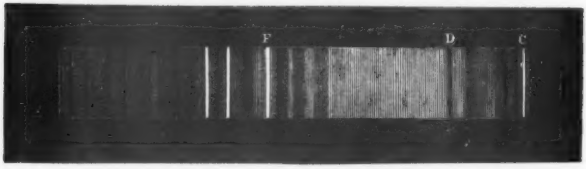
[2] From the Proceedings of the Royal Society, No. 84, 1866.

Borealis. He describes the star as "very brilliant, of about the 2d magnitude." Also Mr. Baxendell of Manchester wrote to one of us, giving the observations which follow of the new star, as seen by him on the night of the 15th instant:

"A new star has suddenly burst forth in Corona. It is somewhat less than a degree distant from ε of that constellation in a southeasterly direction, and last night was fully equal in brilliancy to β Serpentis or ν Herculis, both stars of about the 3d magnitude."

Last night, May 16th, we observed this remarkable object. The star appeared to us considerably below the 3d magnitude, but brighter than ε Coronæ. In the telescope it was surrounded with a faint nebulous haze, extending to a considerable distance, and gradually fading away at the boundary.[3] A comparative examination of neighboring stars showed that this nebulosity really existed about the star. When the spectroscope was placed on the telescope, the light of this new star formed a spectrum unlike that of any celestial body which we have hitherto examined. The light of the star is compound, and has emanated from two different sources. Each light forms its own spectrum. In the instrument these spectra appear superposed. The principal spectrum is analogous to that of the sun, and is evidently formed by the light of an incandescent solid or liquid photosphere, which has suffered absorption by the vapors of an envelope cooler than itself. The second spectrum consists of a few bright lines, which indicate that the light by which it is formed was emitted by matter in the state of luminous gas.[4] These spectra are represented with considerable approximative accuracy in the annexed diagram.

Spectrum of absorption and spectrum of bright lines forming the compound spectrum of a new star near ε Coronæ Borealis.



[3] On the 17th this nebulosity was suspected only; on the 19th and 21st it was not seen.

[4] The position of the groups of dark lines shows that the light of the photosphere, after passing through the absorbent atmosphere, is yellow. The light, however, of the green and blue bright lines makes up to some extent for the green and blue rays (of other refrangibilities) which have been stopped by absorption. To the eye, therefore, the star appears nearly white. However, as the star flickers, there may be noticed an occasional preponderance of yellow or blue. Mr. Baxendell, without knowing the results of prismatic analysis, describes the impression he received to be "as if the yellow of the star were seen through an overlying film of a blue tint."

Description of the spectrum of absorption.—In the red a little more refrangible than Fraunhofer's C are two strong dark lines. The interval between these and a line a little less refrangible than D is shaded by a number of fine lines very near each other. A less strongly marked line is seen about the place of solar D. Between D and a portion of the spectrum about the place of *b* of the solar spectrum, the lines of absorption are numerous, but very thin and faint. A little beyond *b* commences a series of close groups of strong lines; these follow each other at small intervals, as far as the spectrum can be traced.

Description of the gaseous spectrum.—A bright line, much more brilliant than the part of the continuous spectrum upon which it falls, occupies a position which several measures make to be coincident with Fraunhofer's F.[5] At rather more than one-fourth of the distance which separates F and G, a second and less brilliant line was seen. Both these lines were narrow and sharply defined. Beyond these lines, and at a distance a little more than one-third of that which separates the second bright line from the strongest bright one, a third bright line was observed. The appearance of this line suggested that it was either double or undefined at the edges. In the more refrangible part of the spectrum, probably not far from G of the solar spectrum, glimpses were obtained of a fourth and a faint bright line. At the extreme end of the visible part of the less refrangible end of the spectrum, about C, appeared a line brighter than the normal relative brilliancy of this part of the spectrum. The brightness of this line, however, was not nearly so marked in proportion to that of the part of the spectrum where it occurs, as was that of the lines in the green and blue.[6]

General conclusions.—It is difficult to imagine the present physical constitution of this remarkable object. There must be a photosphere of matter in the solid or liquid state emitting light of all refrangibilities. Surrounding this must exist also an at-

[5] On the 17th, the lines of hydrogen, produced by taking the induction-spark through the vapor of water, were compared in the instrument simultaneously with the bright lines of the star. The brightest line coincided with the middle of the expanded line of hydrogen which corresponds to Fraunhofer's F. On account of the faintness of the red end of the spectrum, when the amount of dispersion necessary for these observations was employed, the exact coincidence of the line in this part of the spectrum with the red line of hydrogen, though extremely probable, was not determined with equal certainty.

[6] The spectra of the star were observed again on the 17th, the 19th, the 21st, and the 23d. On these evenings no important alteration had taken place. On the 17th and succeeding evenings, though the spectrum of the waning star was fainter than on the 16th, the red bright line appeared a little brighter relatively to the green and blue bright lines. On the 19th and 21st the absorption lines about *b* were stronger than on the 16th. From the 16th the continuous spectrum diminished in brightness more rapidly than the gaseous spectrum, so than on the 23d, though the spectrum as a whole was faint, the bright lines were brilliant when compared with the continuous spectrum.

mosphere of cooler vapors, which give rise by absorption to the groups of dark lines.

Besides this constitution, which it possesses in common with the sun and the stars, there must exist the source of the gaseous spectrum. That this is not produced by the faint nebulosity seen about the star is evident by the brightness of the lines, and the circumstance that they do not extend in the instrument beyond the boundaries of the continuous spectrum. The gaseous mass from which this light emanates must be at a much higher temperature than the photosphere of the star; otherwise it would appear impossible to explain the great brilliancy of the lines compared with the corresponding parts of the continuous spectrum of the photosphere. The position of two of the bright lines suggests that this gas may consist chiefly of hydrogen.

If, however, hydrogen be really the source of some of the bright lines, the conditions under which the gas emits the light must be different from those to which it has been submitted in terrestrial observations; for it is well known that the line of hydrogen in the green is always fainter and more expanded than the brilliant red line which characterizes the spectrum of this gas. On the other hand, the strong absorption indicated by the line F of the solar spectrum, and the still stronger corresponding lines in some stars, would indicate that under suitable conditions hydrogen may emit a strong luminous radiation of this refrangibility.[1]

The character of the spectrum of this star, taken together with its sudden outburst in brilliancy and its rapid decline in brightness, suggests to us the rather bold speculation that, in consequence of some vast convulsion taking place in this object, large quantities of gas have been evolved from it; that the hydrogen present is burning by combination with some other element and furnishes the light represented by the bright lines; also that the flaming gas has heated to vivid incandescence the solid matter of the photosphere. As the hydrogen becomes exhausted, all the phenomena diminish in intensity, and the star rapidly wanes.

In connexion with this star, the observations which we made upon the spectra of α Orionis and β Pegasi, that they contain no absorption lines of hydrogen, appear to have some new interest. The spectra of these stars agree in their general characters with the absorption spectrum of the new star. The whole class of white stars are distinguished by having hydrogen lines of extraordinary force. It may also be mentioned here that we have found that the spectra of several of the more remarkable of the variable stars, namely, those distinguished by an orange or ruddy

[1] On the dependence of the relative characters of the bright lines of hydrogen upon conditions of pressure and temperature, see Plücker and Hittorf, Phil. Trans., 1865, p. 21.

tint, possess a close general accordance with those of α Orionis, β Pegasi, and the absorption spectrum of the remarkable object described in this paper. The purely speculative idea presents itself from these observations, that hydrogen probably plays an important part in the differences of physical constitution which apparently separate the stars into groups, and possibly also in the changes by which these differences may be brought about.[a]

ART. LIII.—*On the Source of Muscular Power;* by EDWARD FRANKLAND, Ph.D., F.R.S.[1]

WHAT is the source of muscular power? Twenty years ago, if this question had been asked, there were but few philosophers who would have hesitated to reply, "The source of muscular power is that peculiar force which is developed by living animals, and which we term the *vital force!*" but the progress of scientific discovery has rendered the view implied in such an answer so utterly untenable that, at the present moment, no one possessing any knowledge of physical science would venture to return such a reply. We now know that an animal, however high its organization may be, can no more *generate* an amount of force capable of moving a grain of sand, than a stone can fall upwards or a locomotive drive a train without fuel. All that such an animal can do is to liberate that store of force, or *potential energy*, which is locked up in its food. It is the *chemical change* which food suffers in the body of an animal that liberates the previously pent-up forces of that food, which now make their appearance in the form of *actual energy*—as heat and mechanical motion.

From food, and food alone, comes the *matter* of which the animal body is built up; and from food alone come all the different kinds of *physical force* which an animal is capable of manifesting.

The two chief forms of force thus manifested are *Heat* and *Muscular motion* or *mechanical work*, and these have been almost universally traced to two distinct sources—the *heat* to the oxyd-

[a] Mr. Baxendell sends us the following table of magnitudes:

May 15 at	12h	0m	G.M.T.,	T Coronæ	=3·6 or 3·7 magnitude.
" 16 "	10	30	"	"	4·2
" 17 "	11	0	"	"	4·9
" 18 "	12	30	"	"	5·3
" 19 "	12	15	"	"	5·7
" 20 "	12	30	"	"	6·2
" 21 "	12	0	"	"	7·3
" 22 "	11	15	"	"	7·7
" 23 "	10	30	"	"	7·9
" 24 "	10	30	"	"	8·1

[1] From the Proc. Roy. Inst. of Great Britain, June 8, 1866.

ation of the *food*, and the *mechanical work* to the oxydation of the *muscles*.

This doctrine, first promulgated, the speaker believed, by Liebig, occupies a prominent position in that philosopher's justly celebrated 'Chemico-Physiological Essays.'

In his work entitled 'Die organische Chemie in ihrer Anwendung auf Physiologie und Pathologie, Braunschweig, 1842,' Liebig says, "All experience teaches that there is only one source of mechanical power in the organism, and this source is the transformation of the living parts of the body into lifeless compounds. . . . This transformation occurs in consequence of the combination of oxygen with the substance of the living parts of the body." And again, in his 'Letters on Chemistry, 1851,' p. 366, referring to these living parts of the body, he says, "All these organized tissues, all the parts which in any way manifest force in the body are derived from the albumen of the blood; all the albumen of the blood is derived from the plastic or sanguineous constituents of the food, whether animal or vegetable. It is clear, therefore, that the plastic constituents of food, the ultimate source of which is the vegetable kingdom, are the conditions essential to all production or manifestation of force, to all these effects which the animal organism produces by means of its organs of sense, thought, and motion." And again, at page 374, he says, "The sulphurized and nitrogenous constituents of food determine the continuance of the manifestations of force; the non-nitrogenous serve to produce heat. The former are the builders of organs and organized structures, and the producers of force; the latter support the respiratory process, they are *materials for respiration*."

This doctrine has since been treated as an almost self-evident truth in most physiological text-books; it has been quite recently supported by Ranke;[2] and, in his lecture 'On the Food of Man in relation to his Useful Work, 1865,' Playfair says, page 37, "From the considerations which have preceded, we consider Liebig amply justified in viewing the non-nitrogenous portions of food as mere heat-givers. . . . While we have been led to the conclusion that the transformation of the tissues is the source of dynamical power in the animal." At page 30 he also says, "I agree with Draper and others in considering the contraction of a muscle due to a disintegration of its particles, and its relaxation to their restoration. . . . All these facts prove that transformation of the muscle through the agency of oxygen is the condition of muscular action." Finally, in a masterly review of the present relations of chemistry to animal life, published in March last,[3] Odling says, page 98, "Seeing, then, that

[2] 'Tetanus eine Physiologische Studie.' Leipzig, 1865.
[3] 'Lectures on Animal Chemistry.'

muscular exertion is really dependent upon muscular oxydation, we have to consider what should be the products, and what the value of this oxydation." . . . And again, page 103, "The slow oxydation of so much carbon and hydrogen in the human body, therefore, will always produce its due amount of heat, or an equivalent in some other form of energy; for while the latent force liberated by the combustion of the carbon and hydrogen of fat is expressed *solely in the form of heat*, the combustion of an equal quantity of the carbon and hydrogen of voluntary muscle is expressed *chiefly in the form of motion.*"

Nevertheless, this view of the origin of muscular power has not escaped challenge. Immediately after its first promulgation, Dr. J. R. Mayer wrote,[4] "A muscle is only an apparatus by means of which the transformation of force is effected, *but it is not the material by the chemical change of which* mechanical work is produced." He showed that the 15 lbs. of dry muscles of a man weighing 150 lbs. would, if their mechanical work were due to their chemical change, be completely oxydized in eighty days, the heart itself in eight days, and the ventricles of the heart in two and a half days. After endeavoring to prove by physiological arguments that not one per cent of the oxygen absorbed in the lungs could possibly come into contact with the substance of the muscles, Mayer says, "The fire-place in which this combustion goes on is the interior of the blood vessels, the *blood* however—a slowly-burning liquid—is the oil in the flame of life. . . . Just as a plant-leaf transforms a given mechanical effect, *light*, into another force, *chemical difference*, so does the muscle produce mechanical work at the cost of the chemical difference consumed in its capillaries. Heat can neither replace the sun's rays for the plant, nor the chemical process in the animal: every act of motion in an animal is attended by the consumption of oxygen and the production of carbonic acid and water; every muscle to which atmospheric oxygen does not gain access ceases to perform its functions."

But Mayer was not the first to conceive this view of muscular action. Nearly 200 years ago, a Bath physician, Dr. John Mayow,[5] distinctly stated that for the production of muscular motion two things are necessary—the conveyance of combustible substances to the muscle by the blood, and the access of oxygen by respiration. He concluded that the chief combustible substance so used was fat. A century before Priestley isolated oxygen, Mayow was aware of its existence in the air, in nitre, and in nitric acid; he knew that combustion is supported by the oxygen of the air, and that this gas is absorbed in the

[4] 'Die organische Bewegung in ihrem Zusammenhange mit dem Stoffwechsel,' 1845.

[5] 'De Motu musculari,' 1681. Mayow was born in 1645, and died 1679.

lungs by the blood, and is absolutely necessary for muscular activity.

For two decades this doctrine sank into oblivion; and it is only within the last two years that it has been again advanced, chiefly by Haidenhain,[6] Traube, and, to a limited extent, by Donders.[7]

Experimental evidence was, however, still wanting to give permanent vitality to the resuscitated doctrine; for although the laborious and remarkable investigations of Voit[8] and of Edward Smith[9] point unmistakably in the direction of Mayow and Mayer's hypothesis, yet the results of these physiologists were not sufficiently conclusive to render the opposite view untenable. This want of data of a sufficiently conclusive character has been supplied by a happily conceived experiment undertaken by Fick and Wislicenus in the autumn of last year, and described in the 'Philosophical Magazine,' vol. xxxi, p. 485. In the application of these data, however, to the problem now under consideration, one important link was found to be wanting, viz., the amount of actual energy generated by the oxydation of a given weight of muscle in the human body. Fick and Wislicenus refer to this missing link in the following words: "The question now arises what quantity of heat is generated when muscle is burnt to the products in which its constituent elements leave the human body through the lungs and kidneys? At present, unfortunately, there are not the experimental data required to give an accurate answer to this important question, for neither the heat of combustion of muscle nor of the nitrogenous *residue* (urea) of muscle is known." Owing to the want of these data, the numerical results of the experiments of Fick and Wislicenus are rendered less conclusive against the hypothesis of muscle combustion than they otherwise would have been, while similar determinations, which have been made by Edward Smith, Haughton, Playfair, and others, are even liable to a total misinterpretation from the same cause.

The speaker stated that he had supplied this want by the calorimetrical determination of the actual energy evolved by the combustion of muscle and of urea in oxygen. Availing himself of these data he then proceeded to the consideration of the problem to be solved, the present condition of which might be thus summed up:—It is agreed on all hands that muscular

[6] 'Mechanische Leistung Wärmeentwickelung und Stoffumsatz bei der Muskelthätigkeit,' 1864.

[7] As this is passing through the press, the speaker has become aware that Messrs. Lawes and Gilbert advocated this doctrine in 1852, and repeatedly since; their opinions being founded upon experiments on the feeding of cattle.

[8] 'Untersuchungen über den Einfluss des Kochsalzes, des Kaffeés und der Muskelbewegungen auf den Stoffwechsel,' p. 150. Munich, 1860.

[9] Phil. Trans., 1861, p. 747.

power is derived exclusively from the mutual chemical action of the food and atmospheric oxygen; but opinions differ as to whether that food must first be converted into the actual organized substance of the muscle, before its oxydation can give rise to mechanical force, or whether it is not also possible that muscular work may be derived from the oxydation of the food, which has only arrived at the condition of blood and not of organized muscular tissue.

The importance of this problem can scarcely be overrated; it is a corner-stone of the physiological edifice, and the key to the phenomena of the nutrition of animals. For its satisfactory solution the following data require to be determined:

1st. The amount of force or actual energy generated by the oxydation of a given amount of muscle in the body.

2d. The amount of mechanical force exerted by the muscles of the body during a given time.

3d. The quantity of muscle oxydized in the body during the same time.

If the total amount of force involved in muscular action, as measured by the mechanical work performed, be greater than that which could possibly be generated by the quantity of muscle oxydized during the same time, it necessarily follows that the power of the muscles is not derived *exclusively* from the oxydation of their own substance.

As regards the first datum to be determined, it is necessary to agree upon some unit for the measurement of mechanical force. The unit most commonly adopted is that represented by the lifting of a kilogram weight to the height of one meter. The researches of Joule and Mayer have connected this standard unit with heat;—they prove that the force required to elevate this weight 425 times will, when converted into heat, raise the temperature of an equal weight of water 1° C. If this weight were let fall from a height of 425 meters, its collision with the earth would produce an amount of heat sufficient to raise the temperature of 1 kilogram of water 1° C. The same heating effect would also of course be produced by the fall of 425 kilograms through 1 meter. This standard of force is termed a *meterkilogram;*[10] and 425 meterkilograms are equal to that amount of heat which is necessary to raise the temperature of 1 kilogram of water through 1° C. If then it be found that the heat evolved by the combustion of a certain weight of charcoal or muscle, for instance, raises the temperature of 1 kilogram of water through 1° C., this means, when translated into mechanical power, 425 meterkilograms. Again, if a man weighing 64 kilograms climbs to a height of 1,000 meters, the ascent of his

[10] I follow the example of the Registrar-General in abbreviating the French word *gramme* to gram.

body to this height represents 64,000 meterkilograms of work; that is, the labor necessary to raise a kilogram weight to the height of 1 meter 64,000 times.

In order to estimate the amount of actual energy generated by the oxydation of a given amount of muscle in the body, it is necessary to determine, first, the amount of actual energy generated by the combustion of that amount of muscle in oxygen, and then to deduct from the number thus obtained the amount of energy still remaining in the products of the oxydation of this quantity of muscle which leave the body. Of these products, urea and uric and hippuric acids are the only ones in appreciable quantity which still retain potential energy on leaving the body, and of these the two latter are excreted in such small proportions that they may be considered as urea without introducing any material error into the results.

These determinations were made in Lewis Thompson's calorimeter, which consists of a copper tube to contain a mixture of chlorate of potash with the combustible substance, and which can be enclosed in a kind of diving-bell, also of copper, and so lowered to the bottom of a suitable vessel containing a known quantity (2 liters) of water. The determinations were made with this instrument in the following manner:—19·5 grams of chlorate of potash, to which about one-eighth of peroxyd of manganese was added, was intimately mixed with a known weight (generally about 2 grams) of the substance whose potential energy was to be determined, and the mixture being then placed in the copper tube above mentioned, a small piece of cotton thread, previously steeped in chlorate of potash and dried, was inserted in the mixture. The temperature of the water in the calorimeter was now ascertained by a delicate thermometer; and the end of the cotton thread being ignited, the tube with its contents was placed in the copper bell and lowered to the bottom of the water. As soon as the combustion reached the mixture a stream of gases issued from numerous small openings at the lower edge of the bell and rose to the surface of the water—a height of about 10 inches.

At the termination of the deflagration, the water was allowed free access to the interior of the bell, by opening a stop-cock connected with the bell by a small tube rising above the surface of the water in the calorimeter. The gases in the interior of the bell were thus displaced by the incumbent column of water, and by moving the bell up and down repeatedly, a perfect equilibrium of temperature throughout the entire mass of water was quickly established. The temperature of the water was again carefully observed, and the difference between this and the previous observation determines the calorific power or potential energy, expressed as heat, of the substance consumed.

The value thus obtained is, however, obviously subject to the following corrections:—

1. The amount of heat absorbed by the calorimeter and apparatus employed, *to be added.*

2. The amount of heat carried away by the escaping gases, after issuing from the water, *to be added.*

3. The amount of heat due to the decomposition of the chlorate of potash employed, *to be deducted.*

4. The amount of heat equivalent to the work performed by the gases generated in overcoming the pressure of the atmosphere, *to be added.*

Although the errors due to these causes to some extent neutralize each other, there is still an outstanding balance of sufficient importance to require that the necessary corrections should be carefully attended to.

The amount of error from the first cause was once for all experimentally determined, and was added to the increase of temperature observed in each experiment.

The amount of heat carried away by the escaping gases after issuing from the water may be divided into two items, viz.:—

a. The amount of heat rendered latent by the water which is carried off by the gases in the form of vapor.

b. The amount of heat carried off by these gases by reason of their temperature being above that of the water from which they issue.

It was ascertained that a stream of dry air when passed through the water of the calorimeter, at about the same rate and for the same period of time as the gaseous products of combustion, depressed the temperature of the water by only 0°·02 C.

By placing a delicate thermometer in the escaping gases, and another in the water, no appreciable difference of temperature could be observed. Both these items may therefore be safely neglected.

The two remaining corrections can be best considered together, since a single careful determination eliminates both. When a combustible substance is burnt in gaseous oxygen, the conditions are essentially different from those which obtain when the same substance is consumed at the expense of the combined or solid oxygen of chlorate of potash. In the first case the products of combustion, when cooled to the temperature of the water in the calorimeter, occupy less space than the substances concerned in the combustion, and no part of the energy developed is therefore expended in external work, that is, in overcoming the pressure of the atmosphere. In the second case, both the combustible and the supporter of combustion are in the solid condition, whilst a considerable proportion of the products of combustion are gases. The generation of the latter cannot take place without the performance of external work, for every cubic inch pro-

duced must obviously, in overcoming atmospheric pressure, perform an amount of work equivalent, in round numbers, to the lifting of a weight of 15 lbs. to the height of one inch. In performing this work the gases are cooled, and consequently less heat is communicated to the water of the calorimeter. Nevertheless, the loss of heat due to this cause is but small. Under the actual conditions of the experiments detailed below, its amount would only have increased the temperature of the water in the calorimeter by 0°·07 C. Even this slight error is entirely eliminated by the final correction which we have now to consider.

It is well known that the decomposition of chlorate of potash into chlorid of potassium and free oxygen is attended with the evolution of heat. If a few grains of peroxyd of manganese, or better, of peroxyd of iron, be dropped into an ounce or two of fused chlorate of potash which is slowly disengaging oxygen, the evolution of gas immediately proceeds with great violence, and the mixture becomes visibly red hot, although the external application of heat be discontinued from the moment when the metallic peroxyd is added. The latter remains unaltered at the close of the operation. It is thus obvious that chlorate of potash, on being decomposed, furnishes considerably more heat than that which is necessary to gasify the oxygen which it evolves. It was therefore necessary to determine the amount of heat thus evolved by the quantity of chlorate of potash (9·75 grams) mixed with one gram of the substance burnt in each of the following determinations. This was effected by the use of two copper tubes, the one placed within the other. The interior tube was charged with a known weight of the same mixture of chlorate of potash and peroxyd of manganese as that used for the subsequent experiments, whilst the annular space between the two tubes was filled with a combustible mixture of chlorate and spermaceti, the calorific value of which had been previously ascertained. The latter mixture was ignited in the calorimeter as before, and the heat generated during its combustion effected the complete decomposition of the chlorate in the interior cylinder, as was proved by a subsequent examination of the liquid in the calorimeter, which contained no traces of undecomposed chlorate. The following are the results of five experiments thus made, expressed in units of heat, the unit being equal to 1 gram of water raised through 1° C. of temperature:—

	Units of Heat.
1st experiment, - - - - -	340
2nd " - - - - -	300
3rd " - - - - -	375
4th " - - - - -	438
5th " - - - - -	438
	5)1891
Mean, - - - -	378

This result was confirmed by the following experiments:—

1. Starch was burnt, firstly, in a current of oxygen gas, and secondly, by admixture with chlorate of potash and peroxyd of manganese.

Heat units furnished by one gram of starch burnt with 9·75 grams chlorate of potash, - - - - - - -	4290
Heat units furnished by the same weight of starch burnt in a stream of oxygen gas, - - - - - - - -	3964
Difference - - - -	326

2d. Phenylic alcohol was burnt with chlorate of potash, and the result compared with the calorific value of this substance as determined by Favre and Silbermann.

Heat units furnished by one gram of phenylic alcohol burnt with 9·75 grams chlorate of potash, - - - - -	8183
Heat units furnished by one gram of phenylic alcohol when burnt with gaseous oxygen (Favre and Silbermann), - -	7842
Difference, - - - - - -	341

These three determinations of the heat evolved by the decomposition of 9·75 grams of chlorate of potash, furnishing the numbers 378, 326, and 341, agree as closely as could be expected, when it is considered that all experimental errors are necessarily thrown upon the calorific value of the chlorate of potash.

The mean of the above five experimental numbers was, in all cases, deducted from the actual values read off in the following determinations.

It was ascertained by numerous trials that all the chlorate of potash was decomposed in the deflagrations, and that but mere traces of carbonic oxyd were produced.

Joule's mechanical equivalent of heat was employed, viz., 1 kilogram of water raised 1° C.=423 meterkilograms.

The following results were obtained:

Actual energy developed by one gram of each substance when burnt in oxygen.

Name of substance dried at 100° C.	Heat units.					Meter-kilograms of force. (Mean.)
	1st Experiment.	2d Experiment.	3d Experiment.	4th Experiment.	Mean.	
Beef muscle purified by repeated washing with ether,	5174	5062	5195	5088	5103	2161
Purified albumen, ...	5009	4987	....	....	4998	2117
Beef fat, ...	9069	....	....	....	9069	3841
Hippuric acid, ...	5330	5437	....	....	5383	2280
Uric acid, ...	2645	2585	....	....	2615	1108
Urea,[11]	2121	2302	2207	2197	2206	934

[11] The speaker showed the combustibility of urea by burning it upon asbestos in a jar of oxygen gas.

It is evident that the above determination of the actual energy developed by the combustion of muscle in oxygen represents more than the amount of actual energy produced by the oxydation of muscle within the body, because, when muscle burns in oxygen its carbon is converted into carbonic acid, and its hydrogen into water; the nitrogen being, to a great extent, evolved in the elementary state; whereas, when muscle is most completely consumed in the body, the products are carbonic acid, water and urea; the whole of the nitrogen passes out of the body as urea—a substance which still retains a considerable amount of potential energy. Dry muscle and pure albumen yield, under these circumstances, almost exactly one-third of their weight of urea, and this fact, together with the above determination of the actual energy developed on the combustion of urea, enables us to deduce with certainty the amount of actual energy developed by muscle and albumen respectively when consumed in the human body. It is as follows:—

Actual energy developed by one gram of each substance when consumed in the body.

Name of substance dried at 100° C.	Heat units. (Mean.)	Meterkilograms of force. (Mean.)
Beef muscle purified by ether,	4868	1848
Purified albumen, ...	4263	1803

We have thus ascertained the first of our three data, viz., the amount of force or actual energy generated by the oxydation of a given amount of muscle in the body; and we now proceed to ascertain the second, viz., the amount of mechanical force exerted by the muscles of the body during a given time. For this purpose we have only to avail ourselves of the details of Fick and Wislicenus's conclusive experiment already referred to, and which consisted in the ascent of the Faulhorn in Switzerland from the lake of Brienz. This mountain can be ascended by a very steep path from Iseltwald, which was of course favorable for the experiment, and there is a hotel on the summit which allowed the experimenters to pass the following night under tolerably normal circumstances. The following is their own description and estimate of the amount of work performed in the ascent."[19]

"Let us now inquire how much work was really done by our muscles. One item necessary for the reply is already at hand, viz., the height of the summit of the Faulhorn above the level of the lake of Brienz multiplied by the weight of the body; the former reckoned in meters, the latter in kilograms. The weight of the body with the equipments (hat, clothes, stick)

[19] Phil. Mag., vol. xxxi, p. 496, 1866.

amounted to 66 kilograms in Fick's case, and 76 in Wislicenus's. The height above the Faulhorn above the level of the lake of Brienz is, according to trigonometric measurements, exactly 1956 meters. Therefore Fick performed 129,096 and Wislicenus 148,656 meterkilograms of muscular work."

But in addition to this measurable external work there is another item of force "which can be expressed in units of work; and though its value cannot be quite accurately calculated, yet a tolerable approximation can be made. It consists of the force consumed in respiration and the heart's action. The work performed by the heart has been estimated, in a healthy full-grown man, at about 0·64 meterkilogram[13] for each systole. During the ascent, Fick's pulse was about 120 per minute. That gives for the 5·5 hours of the ascent an amount of work which may be estimated at 25,344 meterkilograms, entirely employed in the maintenance of the circulation. No attempt has yet been made to estimate the labor of respiration. One of us has shown, however, in the second edition of his 'Medical Physics' (p. 206), that Donders's well-known investigations concerning the conditions of pressure in the cavity of the thorax give sufficient data for such an estimate. He has there shown that the amount of work performed in an inspiration of 600 cubic centims. may be rated at about 0·63 meterkilogram. Fick breathed during the ascent at an average rate of about 25 respirations per minute, which gives, according to this estimation, an amount of respiratory work for the whole ascent of 5197 meterkilograms. If we add this, and the number representing the work of the heart, to the external work performed by Fick, we obtain a total of 159,637 meterkilograms. If we suppose that Wislicenus's respiratory and circulatory work bore the same proportion to Fick's as his bodily weight did to Fick's, i. e., 7 : 6, we obtain for Wislicenus's amount of work, as far as it is possible to calculate it, a total of 184,287 meterkilograms.

"Besides these estimated (and certainly not over-estimated) items, there are several others which cannot be even approximately calculated, but the sum of which, if it could be obtained, would probably exceed even our present large total. We will try to give at least some sort of an account of them. It must first be remembered that in the steepest mountain path there are occasional level portions, or even descents. In traversing such places the muscles of the leg are exerted as they are in ascending, but the whole work performed is transformed back into heat. The same force-producing process, however, must be going on in the muscles as if work were being performed which

[13] 0·43 is here assigned as the work of the left, and 0·21 as that of the right ventricle.

did not undergo this transformation. In order to make this point yet clearer we may take into consideration that the whole work of the ascent, only existed temporarily as work. On the following day the result was reversed; our bodies approached the center of the earth by as much as they had receded from it the day before, and, in consequence, on the second day an amount of heat was liberated equal to the amount of work previously performed. The two parts of the action, which in this case were performed on two separate days, take place in walking on level ground in the space of a footstep.

"Let us observe, besides, that in an ascent it is not only those muscles of the leg specially devoted to climbing which are exerted, the arms, head, and trunk are continually in motion. For all these movements force-generating processes are necessary, the result of which cannot, however, figure in our total of work, but must appear entirely in the form of heat, since all the mechanical effects of these movements are immediately undone again. If we raise an arm, we immediately let it drop again, &c.

"There was besides a large portion of our muscular system employed during the ascent, which was performing no external work (not even temporary work, or mechanical effects immediately reversed), but which cannot be employed without the same force-generating processes which render external work possible. As long as we hold the body in an upright position, individual groups of muscles (as, for instance, the muscles of the back, neck, &c.) must be maintained in a state of continual tetanus in order to prevent the body from collapsing. We may conceive of a tetanized muscle as holding up a weight which would immediately fall if the supply of actual energy were to cease. It is active, but it performs no work, and therefore all the force produced is liberated in the form of heat."

Thus the total amount of measured and estimable work performed in 5·5 hours in the experiments before us was 159,637 meterkilograms for Fick, and 184,287 meterkilograms for Wislicenus. This is our second datum.

The third, viz., the amount of muscle oxydized in the body during the performance of this work has been carefully determined by the same experimenters, as well as the rate of muscle consumption before and after the ascent. For the details of these determinations the speaker referred his hearers to the Philosophical Magazine for 1866, vol. xxxi, p. 488; but the following is a condensed summary of the results:—

Ascent of the Faulhorn.

	Fick.	Wislicenus.
	Gram.	Gram.
Amount of nitrogen secreted in urine per hour before ascent,	·63	·61
Weight of dry muscle corresponding to nitrogen, ...	4·19	4·05
Amount of nitrogen secreted per hour during ascent,.....	·41	·39
Weight of dry muscle corresponding to nitrogen, ...	2·70	2·56
Amount of nitrogen secreted per hour during 6 hours after the ascent, ...	·40	·40
Weight of dry muscle corresponding to nitrogen, ...	2·63	2·63
Amount of nitrogen secreted per hour during the following night, ...	·45	51
Weight of dry muscle corresponding to nitrogen, ...	3·06	3·39
Total amount of nitrogen secreted during ascent, ...	3·31	3·13
Ditto during 6 hours after ascent, ...	2·43	2·42
	5·74	5·55
Weight of dry muscle corresponding to nitrogen secreted, During ascent, ...	20·98	20·89
Weight of dry muscle corresponding to nitrogen secreted, During 6 hours after ascent,	16·19	16·11
	37·17	37·00

The results of these determinations add a new link to the chain of experimental evidence, that muscular exertion does not necessarily increase the excretion of nitrogen through the urine. From mid-day before the ascent (August 29th, 1865) to the following evening at seven o'clock (August 30th) both gentlemen abstained from all nitrogenous food. During these thirty-one hours they had nothing in the way of solid food except starch, fat, and sugar. The two former were taken in the form of cakes. Starch was made up with water into a thin paste, which was then made into small cakes and fried with plenty of fat. The sugar was taken dissolved in tea. In addition to this there was the sugar contained in the beer and wine, which were taken in quantities usual in mountain excursions. It was doubtless owing to this absence from food containing nitrogen that the amount of this element secreted through the urine, declined tolerably regularly from the 29th of August till the evening of the 30th. Even in the night of the 30th to the 31st, in spite of the plentiful meal of albuminous food on the evening of the 30th, the secretion of nitrogen was less than on the preceding night. The reason of this is probably to be sought for in the circumstance that during the period of abstinence, the secretion of nitrogen was carried on at the expense of tissues, and now these tissues required reparation.

It is perhaps scarcely worthy of record that during the ascent neither of the experimenters perspired perceptibly, since it has been proved by Ranke that no appreciable amount of nitrogen leaves the system in the matter of perspiration; and as Thiry has also shown that no nitrogen is got rid of by respiration, it follows that in addition to the nitrogen contained in the urine,

the only other mode of exit for this element is through the fæces. Now the proportion secreted through the fæces has been estimated by Ranke at about one-twelfth of that in the urine; but inasmuch as all experiments on the subject tend to show that this alvine nitrogen is, as voided, a constituent of un-oxydized compounds, that is, of compounds that have not yielded up their force, it has no claim upon our attention.

There is still another circumstance which requires to be taken into consideration before we proceed to apply our three data to the solution of the problem before us. It is this:—Is it possible that at the termination of the ascent of the Faulhorn there might be a considerable quantity of the nitrogenous products of decomposition retained in the body? Considering the physiological effect of the retention of urea in the system, as exemplified whenever the secretion of urine is interrupted, it is difficult to imagine the possibility of any considerable quantity of urea being retained in the system of a healthy man. It is, however, otherwise with creatin, another of the products of the metamorphosis of tissue; for it has been repeatedly shown that a muscle which has been hard worked contains more creatin than one that has been at rest. Thus the quantity of creatin contained in the heart of an ox was found to be ·14 per cent (Gregory), and that in other ox-flesh only ·06 per cent (Staedeler). Now the muscles which extend the leg in walking, and which do the essential work in ascending, have been estimated by Weber to weigh in both legs 5·8 kilograms, and if we assume that before the ascent these muscles contained ·06 per cent of creatin, while after the ascent the percentage had increased to ·14 per cent, then the amount of creatin thus exceptionally retained would amount to 4·64 grams, which would be derived from 8·4 grams of muscle.

The speaker had been unable to determine the calorific effect of creatin, and consequently the actual energy developed by the transformation of muscle into creatin; for, although he was kindly furnished with an ample supply of this material by Dr. Dittmar, yet all attempts to burn it in the calorimeter were fruitless. Even when mixed in very small proportions with chlorate of potash and other combustibles of known value, the mixture invariably exploded violently on ignition. Although actual determination thus fails us, there can be no doubt that the transformation of muscle into creatin and other non-nitrogenous products must be attended by the liberation of far less actual energy than its transformation into urea, carbonic acid, and water. To be convinced of this, it is only necessary to compare (under equal nitrogen value) the formulæ of muscle, creatin, and urea, remembering at the same time that the nitrogen probably possesses no thermal value, and that each atom of oxygen destroys approximately the thermal effect of two atoms of hydrogen.

	Comparable formulæ.	Powerful or unburnt matter.
Muscle, - -	$C_{24}H_{37}N_6O_7$	$C_{24}H_{23}$
Creatin, - -	$C_8H_{18}N_6O_4$	C_8H_{10}
Urea, - -	$C_2H_{12}N_6O_2$	C_2H_6

Thus it is evident that the amount of creatin exceptionally retained in the system could not greatly affect the result of the experiment as regards the possible amount of actual energy derivable from the metamorphosed tissues during the ascent; firstly, on account of the small quantity of creatin so retained, and, secondly, because creatin still contains about one-third of the potential energy of the muscle from which it is derived. But as this point cannot be experimentally demonstrated, the speaker followed the example of Fick and Wislicenus, and made a very liberal allowance on this score. He allowed, as they had done, that the whole of the nitrogen secreted during the six hours after the ascent was exceptionally retained in the system *as urea* during the ascent. This is equivalent to an admission that the muscles of the legs contained at the end of the ascent eleven times as much creatin as was present in them before the ascent. In the above tabular statement of results provision has been made for this allowance by adding together, on the one hand, the amounts of nitrogen secreted during the ascent and six hours after it, and, on the other, the weights of dry muscle corresponding to these two amounts of nitrogen.

Having thus far cleared the ground, let us now compare the amount of measured and calculated work performed by each of the experimenters during the ascent of the Faulhorn, with the actual energy capable of being developed by the maximum amount of muscle that could have been consumed in their bodies, this amount being represented by the total quantity of nitrogen excreted in each case during the ascent and for six hours afterwards.

	Fick.	Wislicenus.
Weight of dry muscle consumed,	Grams. 37·17	Grams. 37·00
Actual energy capable of being produced by the consumption of 37·17 and 37·00 grams of dry muscle in the body,	Meterkilograms. 68,690	Meterkilograms. 68,376
Measured work performed in the ascent (external work),	129,096	148,656
Calculated circulatory and respiratory work performed during the ascent (internal work),	30,541	35,631
Total ascertainable work performed,	159,637	184,287

It is thus evident that the muscular power expended by these gentlemen in the ascent of the Faulhorn could not be exclusively derived from the oxydation, either of their muscles, or of other nitrogenous constituents of their bodies, since the maximum of

power capable of being derived from this source even under very favorable assumptions is, in both cases, less than one-half of the work actually performed. But the deficiency becomes much greater if we take into consideration the fact, that the actual energy developed by oxydation or combustion cannot be wholly transformed into mechanical work. In the best constructed steam-engine, for instance, only one-tenth of the actual energy developed by the burning fuel can be obtained in the form of mechanical power; and in the case of man, Helmholtz estimates that not more than one-fifth of the actual energy developed in the body can be made to appear as external work. The experiments of Haidenhain, however, show that, under favorable circumstances, a muscle may be made to yield, in the shape of mechanical work, as much as one-half of the actual energy developed within it, the remainder taking the form of heat. Taking then this highest estimate of the proportion of mechanical work capable of being got out of actual energy, it becomes necessary to multiply by two the above numbers representing the ascertainable work performed, in order to express the actual energy involved in the production of that work. We then get the following comparison of the actual energy capable of being developed by the amount of muscle consumed, with the actual energy necessary for the performance of the work executed in the ascent of the Faulhorn.

	Fick.	Wislicenus.
	Meterkilograms.	Meterkilograms.
Actual energy capable of being produced by muscle metamorphosis, ...	68,690	68,376
Actual energy expended in work performed, ...	319,274	368,574

Thus, taking the average of the two experiments, it is evident that *scarcely one-fifth of the actual energy required for the work performed could be obtained from the amount of muscle consumed.*

Interpreted in the same way, previous experiments of a like kind prove the same thing, though not quite so conclusively. To illustrate this I will here give a summary of three sets of experiments: the first, made by Dr. E. Smith, upon prisoners engaged in treadmill labor; the second, by the Rev. Dr. Haughton, upon military prisoners engaged in shot drill; and the third, adduced by Playfair and made upon pedestrians, pile-drivers, men turning a winch, and other laborers.

Treadwheel experiments.—A treadwheel is a revolving drum with steps placed at distances of eight inches, and the prisoners are required to turn the wheel downwards by stepping upwards. Four prisoners, designated below as A, B, C, and D, were employed in these experiments, and each worked upon the wheel in alternate quarters of an hour, resting in a sitting posture during the intervening quarters. The period of actual daily labor

was 3½ hours. The total ascent per hour 2160 feet, or per day 1·432 mile. The following are the results:—

Treadwheel work.—(E. Smith.)

	Weight in kilograms.	Ascent in meters.	Days occupied in ascent.	External work performed in meterkilograms.	Total nitrogen evolved.	Weight of dry muscle corresponding to nitrogen.
					Grams.	Grams.
A	47·6	23,045	10	1,096,942	171·3	1101·2
B	49	23,045	10	1,129,205	174·5	1121·7
C	55	20,741	9	1,140,755	168·0	1080·1
D	56	20,741	9	1,161,496	159·3	1024·3

In these experiments the measured work was performed in the short space of 3½ hours, while the nitrogen estimated was that voided in the shape of urea in 24 hours. It will, therefore, be necessary to add to the measured work that calculated for respiration and circulation for the whole period of 24 hours. This amount of internal work was computed, from the estimates of Helmholtz and Fick, to be as follows:—

Internal work.—(Helmholtz and Fick.)

	Work performed.	Actual energy required.
	Meterkilograms.	Meterkilograms.
Circulation of the blood during 24 hours, at 75 pulsations per minute,	69,120[14]	138,240
Respiration for 24 hours, at 12 respirations per minute,	10,886	21,772
Statical activity of muscles,	not determined.	not determined.
Peristaltic motion,	" "	" "
	80,006	160,012

Taking this estimate for internal work, the average results of the treadwheel experiments may be thus expressed:—

Treadwheel work.

Average external work per man per day, - - -	119,605 mks.
Average nitrogen evolved per man per day, - - -	17·7 grams.
Weight of dry muscle corresponding to average nitrogen evolved per day, - - - - - - - -	114 "
Actual energy producible by the consumption of 114 grams of dry muscle in the body, - - - - - -	210,672 mks.
Average actual energy developed in the body of each man, viz.—	
External work, - - - 119,605×2=239,210 mks.	
Circulation, - - - - 69,120×2=138,240 "	
Respiration, - - - - 10,886×2= 21,772 "	
	399,222 "

In these experiments the conditions were obviously very unfavorable for the comparison of the amount of actual energy producible from muscle metamorphosis, with the quantity of actual energy expended in the performance of estimable work;

[14] Since making use of this number, I find that Donders estimates the work of the heart alone, for 24 hours, at 86,000 meterkilograms, a figure which is higher than that above for the combined work of circulation and respiration.

since, during that portion of the twenty-four hours not occupied in the actual experiment, a large amount of unestimable internal work, such as the statical activity of the muscles, peristaltic motion, &c., was being performed. Nevertheless, these experiments show that the average actual energy developed in producing work in the body of each man was nearly twice as great as that which could possibly be produced by the whole of the nitrogenous matter oxydized in the body during 24 hours. It must also be remarked that the prisoners were fed upon a nitrogenous diet containing six ounces of cooked meat, without bone; a diet which, as is well known, would favor the production of urea.

Shot-drill experiments.—The men employed for these experiments were fed exclusively upon vegetable diet, and they consequently secreted a considerably smaller amount of nitrogen than the flesh-eaters engaged in the treadwheel work. The other conditions were, however, equally unfavorable for showing the excess of work performed, over the amount derivable from muscle metamorphosis.

In shot-drill, each man lifts a 32 lb. shot from a tressel to his breast, a height of 3 feet; he then carries it a distance of 9 feet, and lays it down on a similar support, returning unloaded. Six of these double journeys occupy one minute. The men were daily engaged with—shot-drill 3 hours, ordinary drill 1¼ hours, oakum picking 3½ hours.

The total average daily external work was estimated by Haughton at 96,316 meterkilograms per man.

The following is a condensed summary of the results of these experiments:—

Military vegetarian prisoners at shot-drill.—(Haughton.)

Average external work per man per day,		96,316 mks.
Average nitrogen evolved per man per day,		12·1 grams.
Weight of dry muscle corresponding to average nitrogen evolved per day,		77·9 "
Actual energy producible by the consumption of 77·9 grams of dry muscle in the body,		143,950 mks.
Average actual energy developed daily in the body of each man, viz., External work, 96,316×2=	192,632 mks.	
Internal work,	160,012 "	
		352,644 mks.

Owing chiefly to the vegetable diet of these prisoners, the result is more conclusive than that obtained upon the treadwheel, the amount of work actually performed being considerably more than twice as great as that which could possibly be obtained through the muscle metamorphosis occurring in the bodies of the prisoners.

Playfair's determinations.—In these determinations the number 109,496 meterkilograms was obtained as the average amount

of daily work performed by pedestrians, pile-drivers, porters, paviors, &c.; but, as the amount of muscle consumption is calculated from the nitrogen taken in the food, the conditions are as unfavorable as possible with regard to the point the speaker was seeking to establish; for it is here assumed, not only that all the nitrogen taken in the food enters the blood, but also that it is converted into muscle, and is afterwards oxydized to carbonic acid, water, and urea. The following are the results expressed as in the previous cases:—

Hard-worked laborer.—(Playfair.)

	Work performed.	Actual energy required.
Daily labor (external work), - - -	109,496 mks.	218,992 mks.
Internal work, - - - - - - -	80,006 "	160,012 "
	189,502 mks.	379,004 mks.
Actual energy capable of being produced from 5·5 oz. (155·92 grams) of flesh-formers contained in the daily food of the laborer, -	....	288,140 mks.

Thus, even under the extremely unfavorable conditions of these determinations, the actual work performed exceeded that which could possibly be produced through the oxydation of the nitrogenous constituents of the daily food by more than 30 per cent.

We have seen, therefore, in the above four sets of experiments, interpreted by the data afforded by the combustion of muscle and urea in oxygen, that the transformation of tissue alone cannot account for more than a small fraction of the muscular power developed by animals; in fact, this transformation goes on at a rate almost entirely independent of the amount of muscular power developed. If the mechanical work of an animal be doubled or trebled there is no corresponding increase of nitrogen in the secretions; whilst it was proved on the other hand by Lawes and Gilbert, as early as the year 1854, that animals, under the same conditions as regarded exercise, had the amount of nitrogen in their secretions increased twofold by merely doubling the amount of nitrogen in their food. Whence then comes the muscular power of animals? What are the substances which, by their oxydation in the body, furnish the actual energy, whereof a part is converted into muscular work? In the light of the experimental results detailed above, can it be doubted that a large proportion of the muscular power developed in the bodies of animals has its origin in the oxydation of non-nitrogenous substances? For while the secretion of nitrogen remains nearly stationary under widely different degrees of muscular exertion, the production of carbonic acid increases most markedly with every augmentation of muscular work, as is shown by the following tabulated results of E. Smith's highly important experi-

ments regarding the amount of carbonic acid evolved from his own lungs under different circumstances.[15]

Excretion of carbonic acid during rest and muscular exertion :—

	Carbonic acid per hour.
During sleep,	19·0 grams.
Lying down and sleep approaching,	23·0 "
In a sitting posture,	29·0 "
Walking at rate of 2 miles per hour,	70·5 "
" " 3 " "	100·6 "
On the treadwheel, ascending at the rate of 28·65 feet per minute,	189·6 "

It has been already stated as a proposition upon which all are agreed, that food, and food alone, is the ultimate source from which muscular power is derived; but the above determinations and considerations, the speaker believed, prove conclusively, firstly, that the non-nitrogenous constituents of the food, such as starch, fat, &c., are the chief sources of the actual energy, which becomes partially transformed into muscular work; and secondly, that the food does not require to become organized tissue before its metamorphosis can be rendered available for muscular power; its digestion and assimilation into the circulating fluid—the blood—being all that is necessary for this purpose. It is, however, by no means the non-nitrogenous portions of food alone that are capable of being so employed, the nitrogenous also, inasmuch as they are combustible, and consequently capable of furnishing actual energy, might be expected to be available for the same purpose, and such an expectation is confirmed by the experiments of Savory upon rats,[16] in which it is proved that these animals can live for weeks in good health upon food consisting almost exclusively of muscular fibre. Even supposing these rats to have performed no external work, nearly the whole of their internal muscular work must have had its source in the actual energy developed by the oxydation of their strictly nitrogenous food.

It can scarcely be doubted, however, that the chief use of the nitrogenous constituents of food is for the renewal of muscular tissue; the latter, like every other part of the body, requiring a continuous change of substance, while the chief function of the non-nitrogenous is to furnish by their oxydation the actual energy which is in part transmuted into muscular force.

The combustible food and oxygen coëxist in the blood which courses through the muscle, but when the muscle is at rest there is no chemical action between them. A command is sent from the brain to the muscle, the nervous agent determines oxydation. The potential energy becomes active energy, one portion assuming the form of motion, another appearing as heat. *Here is the*

[15] Phil. Trans. for 1859, p. 709. [16] The Lancet, 1863, pages 381 and 412.

source of animal heat, here the origin of muscular power! Like the piston and cylinder of a steam-engine, the muscle itself is only a machine for the transformation of heat into motion; both are subject to wear and tear and require renewal, but neither contributes in any important degree by its own oxydation to the actual production of the mechanical power which it exerts.

From this point of view it is interesting to examine the various articles of food in common use, as to their capabilities for the production of muscular power. The speaker had therefore made careful estimations of the calorific value of different materials used as food, by the same apparatus and in the same manner as described above for the determination of the actual energy in muscle, urea, uric acid, and hippuric acid.

The results are embodied in the following series of tables, but it must be borne in mind that it is only on the condition that the food is digested and passes into the blood, that the results given in these tables are realized. If, for instance, sawdust or paraffin oil had been experimented upon, numbers would have been obtained for these substances, the one about equal to that assigned to starch, and the other surpassing that of any article in the table; but these numbers would obviously have been utterly fallacious, inasmuch as neither sawdust nor paraffin oil is, to any appreciable extent, digested in the alimentary canal. While the force-values experimentally obtained for the different articles in these tables must therefore be understood as the maxima assignable to the substances to which they belong, yet it must not be forgotten that a large majority of these substances appear to be completely digestible under normal circumstances.

Actual energy developed by one gram of various articles of food when burnt in oxygen.

Name of food.	Heat units.		Meterkilograms of force.		Per cent of water.
	Dry.	Natural condition.	Dry.	Natural condition.	
Cheese (Cheshire),	6114	4647	2589	1969	24·0
Potatoes,	3752	1013	1589	429	73·0
Apples,	3669	660	1554	280	82·0
Oatmeal,	....	4004	....	1696	...
Flour,	....	3941	....	1669	...
Pea-meal,	....	3936	....	1667	...
Ground rice,	....	3813	....	1615	...
Arrowroot,	....	3912	....	1657	...
Bread crumb,	3984	2231	1687	945	44·0
" crust,	....	4459	....	1888	...
Beef (lean),	5313	1567	2250	664	70·5
Veal "	4514	1314	1912	556	70·9
Ham "	4343	1980	1839	839	54·4
Mackerel,	6064	1789	2568	758	70·5
Whiting,	4520	904	1914	383	80·0
White of egg,	4896	671	2074	284	86·3
Hard-boiled egg,	6821	2383	2677	1009	62·3

Actual energy developed by one gram of various articles of food—continued.

Name of food.	Heat units.		Meterkilograms of force.		Per cent of water.
	Dry.	Natural condition.	Dry.	Natural condition.	
Yolk of egg,	6460	3423	2737	1449	47·0
Gelatin,	4520	...	1914	....	...
Milk,	5093	662	2157	280	87·0
Carrots,	3767	527	1595	223	86·0
Cabbage,	3776	434	1599	184	88·5
Cocoa nibs,	....	6873	....	2911	...
Beef fat,	9069	....	3841	....	...
Butter,	....	7264	....	3077	...
Cod-liver oil,	....	9107	....	3857	...
Lump sugar,	....	3348	....	1418	...
Commercial grape sugar,	....	3277	....	1388	...
Bass's ale (alcohol reckoned),	3776	775	1599	328	88·4
Guinness's stout,	6348	1076	2688	445	88·4

Actual energy developed by one gram of various articles of food when oxydized in the body.

Name of food.	Meterkilograms of force.		Name of food.	Meterkilograms of force.	
	Dry.	Natural condition.		Dry.	Natural condition.
Cheshire cheese,	2429	1846	Hard-boiled egg,	2562	966
Potatoes,	1563	422	Yolk of egg,	2641	1400
Apples,	1516	273	Gelatin,	1550	....
Oatmeal,	....	1665	Milk,	2046	266
Flour,	....	1627	Carrots,	1574	220
Pea-meal,	....	1598	Cabbage,	1543	178
Ground rice,	....	1591	Cocoa nibs,	....	2902
Arrowroot,	....	1657	Butter,	....	3077
Bread crumb,	1625	910	Beef fat,	3841	....
Lean of beef,	2047	604	Cod-liver oil,	3857	....
" veal,	1704	496	Lump sugar,	....	1418
" ham, boiled,	1559	711	Commercial grape sugar,	....	1388
Mackerel,	2315	683	Bass's ale, bottled,	1559	328
Whiting,	1675	335	Guinness's stout,	2688	455
White of egg,	1781	244			

Weight and cost of various articles of food required to be oxydized in the body in order to raise 140 *lbs. to the height of* 10,000 *feet.*

External work = $\frac{1}{5}$th actual energy.

Name of food.	Weight in lbs. required.	Price per lb.	Cost.
		s. *d.*	*s.* *d.*
Cheshire cheese,	1·156	0 10	0 11½
Potatoes,	5·068	0 1	0 5¼
Apples,	7·815	0 1½	0 11¾
Oatmeal,	1·281	0 2¾	0 3½
Flour,	1·311	0 2¾	0 3¾
Pea-meal,	1·335	0 3½	0 4½
Ground rice,	1·341	0 4	0 5½
Arrowroot,	1·287	1 0	1 3½
Bread,	2·345	0 2	0 4¾
Lean beef,	3·532	1 0	3 6½
" veal,	4·300	1 0	4 3½

Weight and cost of various articles of food—continued.

Name of food.	Weight in lbs. required.	Price per lb.	Cost.
		s. d.	*s. d.*
Lean ham, boiled,	3·001	1 6	4 6
Mackerel,	3·124	0 8	2 1
Whiting,	6·369	1 4	9 4
White of egg,	8·745	0 6	4 4½
Hard-boiled egg,	2·209	0 6½	1 2½
Isinglass,	1·377	16 0	22 0½
Milk,	8·021	5*d.* per quart.	1 3½
Carrots,	9·685	0 1½	1 2½
Cabbage,	12·020	0 1	1 0½
Cocoa-nibs,	0·735	1 6	1 1½
Butter,	0·693	1 6	1 0½
Beef fat,	0·555	0 10	0 5½
Cod-liver oil,	0·553	3 6	1 11½
Lump sugar,	1·505	0 6	1 3
Commercial grape sugar,	1·537	0 3½	0 5½
Bass's pale ale (bottled),	9 bottles.	0 10	7 6
Guinness's stout,	6¼ "	0 10	5 7½

Weight of various articles of food required to sustain respiration and circulation in the body of an average man during 24 hours.

Name of food.	Weight in oz.	Name of food.	Weight in oz.
Cheshire cheese,	3·0	Whiting,	16·8
Potatoes,	13·4	White of egg,	28·1
Apples,	20·7	Hard-boiled egg,	5·8
Oatmeal,	3·4	Gelatine,	3·6
Flour,	3·5	Milk,	21·2
Pea-meal,	3·5	Carrots,	25·6
Ground rice,	3·6	Cabbage,	31·8
Arrowroot,	3·4	Cocoa-nibs,	1·9
Bread,	6·4	Butter,	1·8
Lean beef,	9·3	Cod-liver oil,	1·5
" veal,	11·4	Lump sugar,	3·9
" ham, boiled,	7·9	Commercial grape sugar,	4·0
Mackerel,	8·3		

These results are in many instances fully borne out by experience. The food of the agricultural laborers in Lancashire contains a large proportion of fat. Besides the very fat bacon which constitutes their animal food proper, they consume large quantities of so-called apple dumplings, the chief portion of which consists of paste in which dripping and suet are large ingredients, in fact these dumplings frequently contain no fruit at all. Egg and bacon pies and potato pies are also very common *pièces de résistance* during harvest-time, and whenever very hard work is required from the men. The speaker well remembers being profoundly impressed with the dinners of the navigators employed in the construction of the Lancaster and Preston Railway; they consisted of thick slices of bread surmounted with massive blocks of bacon, in which mere streaks of lean were visible. Dr. Piccard states that the Chamois hunters of Western Switzerland are accustomed, when starting on long and fatiguing

expeditions, to take with them, as provisions, nothing but bacon-fat and sugar, because, as they say, these substances are more nourishing than meat. They doubtless find that in fat and sugar they can most conveniently carry with them a store of force-producing matter. The above tables affirm the same thing. They show that ·55 lb. of fat will perform the work of 1·15 lb. cheese, 5 lbs. potatoes, 1·3 lb. of flour or pea-meal or of 3½ lbs. of lean beef. Donders, in his admirable pamphlet 'On the Constituents of Food and their Relation to Muscular Work and Animal Heat,' mentions the observations of Dr. M. C. Verloren on the food of insects. The latter remarks, "Many insects use during a period in which very little muscular work is performed food containing chiefly albuminous matter; on the contrary, at a time when the muscular work is very considerable, they live exclusively, or almost exclusively, on food free from nitrogen." He also mentions bees and butterflies as instances of insects performing enormous muscular work, and subsisting upon a diet containing but the merest traces of nitrogen.

We thus arrive at the following conclusions:—

1. The muscle is a machine for the conversion of potential energy into mechanical force.

2. The mechanical force of the muscles is derived chiefly, if not entirely, from the oxydation of matters contained in the blood, and not from the oxydation of the muscles themselves.

3. In man the chief materials used for the production of muscular power are non-nitrogenous; but nitrogenous matters can also be employed for the same purpose, and hence the greatly increased evolution of nitrogen under the influence of a flesh diet, even with no greater muscular exertion.

4. Like every other part of the body, the muscles are constantly being renewed; but this renewal is not perceptibly more rapid during great muscular activity than during comparative quiescence.

5. After the supply of sufficient albuminized matters in the food of man to provide for the necessary renewal of the tissues, the best materials for the production, both of internal and external work, are non-nitrogenous matters, such as oil, fat, sugar, starch, gum, &c.

6. The non-nitrogenous matters of food, which find their way into the blood, yield up all their potential energy as actual energy; the nitrogenous matters, on the other hand, leave the body with a portion (one-seventh) of their potential energy unexpended.

7. The transformation of potential energy into muscular power is necessarily accompanied by the production of heat within the body, even when the muscular power is exerted externally. This is doubtless the chief and, probably, the only source of animal heat.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

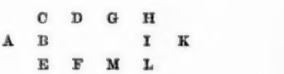
1. *Apparatus for the direct determination of the velocity of sound in atmospheric air;* by Dr. E. C. O. NEUMANN.—This ingenious little apparatus consists of a box of wood (82 c.m. long, 66 c.m. wide, 7 c.m. high) divided by vertical partitions in such a manner as to form two canals, running from one corner, F, of the box, the first only to the middle, the other by a winding course to near the same point so as to be about six meters longer. A little gun is placed on top of the box; when fired the sound is conveyed through a square tube to the corner F, where it divides; one part goes directly to the middle, A, of the front side of the box, and pushing a short wire, *f*, fastened upon a thin caoutchouc membrane by means of a small piece of wood; the other part of the sound traverses the six meters of winding tube, and finally strikes the membrane, closing an aperture, B (near A)—the latter membrane likewise being provided with a blackened wire, *g* (1 mm. diam.). In front of these wires a disk, covered with white paper, can be made to rotate around a horizontal axis.

When this disk is at rest the report passing in at F will soon push the wire *f* against the paper, leaving a black spot—and when the six meters have been traversed, the other part of the sound wave will push the wire *g* against the disk, in the same distance of 15 c.m. from the axis of rotation. But when the disk made one turn per second, these two marks on the circumference were $16\frac{1}{3}$ mm. farther apart than when the disk was at rest. Then we evidently have the velocity, *v*, of sound by the proportion

$$v : 2\pi 15\text{c.m.} :: 6\text{m.} : 16\tfrac{1}{3}\text{mm.},$$

or $v = 346^{m}\cdot 2$. The temperature was 22° C.; a good approximation for a first trial.—*Pogg. Ann.*, 1866, cxxiii, 307–311. G. H.

2. *Interference apparatus for sound waves;* G. QUINCKE.—This apparatus is based upon Herschel's idea of applying branching tubes, and admits of subjective and objective experimentation; it also may be applied to the study of secondary tones (ober-tone), etc., like Helmholtz's resonators. The simplest kind consists of two bent glass tubes DCBEF and GHILM provided with a branch BA and IK. At A is a short rubber tube, which is put into one ear while the other is well closed; at K is another rubber tube leading to a source of sound, viz., the vibrating branch of a tuning fork or its middle rod, or into the box of a monochord, etc. D and G are connected by a short rubber tube; between F and M a longer one is inserted, so as to make ILEB an uneven number of half wave-lengths longer than IHCB. If a tuning fork is used, and the length of MF properly adjusted (the interference-tube tuned), then only the octave is heard, the prime being destroyed by interference. If the rubber tube FM or DG is closed by pressure with the finger, the fundamental tone of the tuning fork is heard again.



For objective representation A is connected with a glass bell closed by a fine membrane, the bell and membrane tuned in unison with the tone experimented with; the sand on the membrane will not be moved when the tone-wave passes through both branches, but it will move as soon as one of the branches is closed.

The same apparatus may also be connected with one of Kundt's tubes described at p. 258 of this volume; and it has finally the great advantage that it can be very easily made by almost anyone.—*Pogg. Ann.*, 1866, cxxviii, 177–192. G. H.

3. *A new apparatus for the demonstration of the laws of falling bodies.* ("Fall-machine;" might it not be rendered "fall-apparatus?")—F. Lippich of Gratz, Austria, has constructed an elegant, simple, and compendious little apparatus for the above purpose, admitting of a much higher degree of accuracy than either Atwood's or Morin's apparatus, and at the same time furnishing us with a drawing representing the laws.

It consists essentially of four parts. A vertical *support* (about 20 inches high) which can be attached to a table; by means of a fine string a *fall frame* (about 15 inches high) is suspended to the same; an elastic vertical *spring*, attached to the support by passing through its position of equilibrium, opens the very ingenious *clamp* holding the string, so that the frame commences its descent at the very moment the spring passes the vertical position. The light frame is covered with well-stretched sooted paper; the vertical spring oscillating parallel to this paper carries a fine point, which marks a wave-line on the descending paper. The oscillations of the spring giving equal times, the intersections of the above curve with the vertical will, and do very accurately, give the spaces described in the times 1, 2, 3, etc. Drawing tangents at these points of intersection the velocities are determined. Measuring the exact length of the fall during six oscillations gave $296{\cdot}24^{mm}$, $296{\cdot}05^{mm}$, $296{\cdot}26^{mm}$, in three experiments. The spring vibrating four times for about fourteen seconds in front of a uniformly moving paper strip, gave a mean of 24·414 vibrations per second. These values give directly $g=9{\cdot}80935$, 9·81007, and 9·80310, or the mean $9{\cdot}80751^{m}$, while the value calculated from $g=9{\cdot}80557\ (1-0{\cdot}002588\cos 2\varphi)$ for Prague, where the experiments were made, and $\varphi=50°\ 5'\ 19''{\cdot}22$, give $g=9{\cdot}81005^{m}$.

Lippich has also experimented upon the influence of the resistance of the air by attaching horizontal card-boards of various sizes to the fall-frame; and finally refers to an apparatus of Laborde based upon the same idea, but less perfect in its realization.—*Sitzungsberichte*, Wien, 1865, II Abth., Bd. lii, p. 549–562; *L'Institut*, 1866, p. 199. G. H.

1. *Astro-photometer and results obtained;* by Dr. J. C. F. Zöllner.—The astro-photometer is described in Zöllner's "Grundzüge einer allgemeinen Photometrie des Himmels, Berlin, 1861." It is polarizing, making use of the law of the square of the cosine as the intensity transmitted through a Nicol. The light of a kerosene-flame, F, passes through a round opening, *a*, to a biconcave lens, *b*, thence to a Nicol *c*, quartz-plate, *f*, Nicol's *d* and *e*, through a double convex lens, *g*, to a plane glass plate inclined 45° toward these rays and the axis of the telescope, O*k*, reflecting the image of *a* to *i* near that of the star seen in the telescope at *k*. The rotation of Nicol *c* is read off on the graduated circle *m*,

that of d on l; by rotation of c the *color*, by turning of d the intensity of the flame is made equal to that of the star. This photometer can be attached to most telescopes.

In his "Photometrische Untersuchungen mit besonderer Rücksicht auf die physische Beschaffenheit der Himmelskörper, Leipzig, 1865," Zöllner has given the *results* of his observations together with their bearing upon the theory of Kant-Laplace. The following intensities were obtained by comparing the sun or planets separately with α Aurigæ; he found

Sun : Capella : : 55,760,000,000 : 1

with a probable error of about 5 per cent; and hence for the intensity at the mean opposition,

		Prob. error.
Sun =	6,994,000,000 times Mars,	5·8 p. c.
Sun =	5,472,000,000 " Jupiter,	5·7 "
Sun =	130,980,000,000 " Saturn (without the ring)	5·0 "
Sun =	8,486,000,000,000 times Uranus,	6 0 "
Sun =	79,620,000,000,000 " Neptune,	5·5 "
Sun =	619,600 " Full Moon,	2·7 "

and by comparing surfaces, Sun = 618,000 times Full Moon, 1·6 p. c.

From the above it follows, that our sun at a distance of 3·72 years-way of light would appear like Capella with a parallax of 0·874 seconds. Peters has actually found 0″·046. If light suffers no absorption in the celestial spaces, Capella accordingly must send out much more light than our sun; and α Centauri seems to be equal to our sun.

The reflecting power or albedo Zöllner found as follows:

		Prob. error.			Prob. error.
Moon,	0·1736	±0·0035	Saturn,	0·4981	±0·0249
Mars,	0·2672	±0·0155	Uranus,	0·6400	±0·0544
Jupiter,	0·6238	±0·0355	Neptune,	0·4648	±0·0372

For the sake of comparison we add his determination of the albedo of terrestrial substances: (*a.*) diffuse reflected light—snow just fallen 0·783, white paper 0·700, white sandstone 0·237, clay-marl 0·156, quartz-porphyry 0·108, moist soil 0·079, dark gray syenite 0·078. (*b.*) regular reflection—mercury 0·648, speculum metal 0·535, glass 0·040, obsidian 0·032, water 0·021.

In regard to the intensity of lunar light in the different phases we must refer for the theoretical investigation to Zöllner's paper in Pogg. Ann., 1866, vol. cxxviii, pp. 46–61; and for the results to pp. 260–262.

In his interesting theoretical views Zöllner distinguishes five periods in the history of any star-sun; viz., 1st, *glowing-gaseous* condition; 2d, *glowing-liquid;* 3d, *slag-period*, gradual development of a cool non-luminous surface; 4th, *eruption-period*, bursting of the cool and dark shell, and 5th, *period of complete refrigeration.* These periods he finds again in the cosmical history of the earth, and in the present aspect of the starry heavens. As representatives of the first period he considers the planetary nebulæ; the other nebulæ stand between the first and second period, which latter is represented by the invariable stars; our *sun* is in the third period; to the fourth belong the new stars; and the fifth period is represented by Bessel's dark stars. For a full exposition of these hypotheses we must refer to the above-mentioned works of Zöllner. G. H.

II. MINERALOGY AND GEOLOGY.

1. *Note on the possible identity of Turnerite with Monazite;* by J. D. DANA.—The crystals of the rare mineral Turnerite have been measured by Levy (who first described the species), Marignac, Phillips, Descloizeaux, and vom Rath. The latest investigations, by Dr. G. vom Rath, are published in Poggendorff's Annalen, vol. cxix, p. 247, and are accompanied with two new figures; his crystals were from a new locality in the Tavetsch valley, at Santa Brigritta near Ruäras—the specimens before known having come from Mt. Sorel in Dauphiny. The crystals are somewhat tabular, with (1) a zone parallel to the orthodiagonal of the three planes, in order, x, c, u, and a fourth a but only as a result of cleavage, on the edge $x : u'$; and (2) a transverse zone, directly across c, containing, either side of c, the planes n, v, e, o, b, the last the face i-i (($\alpha P \alpha$) of Naumann) parallel to the clinodiagonal section; also (3) some other planes. Vom Rath makes $c = O$(oP of Naumann); a (cleavage face) $= i$-i ($\alpha P \alpha$); $u = -1$-i ($-P \alpha$); $x = 1$-i ($+P \alpha$); n, v, e, o, the clinodomes $\frac{1}{3}$-i, $\frac{1}{2}$-i, 1-i, 2-i.

The following are a few of the angles given:

		v. Rath.	Descl.	Marignac.
$a : x$	=	130° 3′	....	....
$a : u$	=	142° 15′	....	....
$c : u$	=	140° 27′	140° 40′	....
$c : x$	=	127° 15′	126° 31′	126° 31′
$c : e$	=	136° 55′	136° 48′	136° 43′

In form and habit the crystals are much like those of monazite, and there is also a close approximation to that species in angle. By changing the position of the crystal so that $a = O$ and $c = i$-i, we have that usually given to the crystals of monazite. The *cleavage face* corresponds to O of monazite (see my Mineralogy, p. 402); $c = i$-i, $u = 1$-i, $x = -1$-i; n, v, e, o, are vertical prisms; and $e = I$, or the fundamental prism. The angles in monazite corresponding to the above are as follows:

$O : -1$-i	=	$a : x$	=	136° 6′	
O : 1-i	=	$a : u$	=	143° 6′	
ii : 1-i	=	$c : u$	=	140° 40′	
ii : -1-i	=	$c : x$	=	126° 8′	(127° 0′, Descl.)
$u : I$	=	$c : e$	=	136° 40′	(136° 30′, Descl.)

The angles cited are sufficient to determine all the dimensions of the crystals; and the approximations in angle and cleavage leave little doubt of at least the near identity in crystallization of turnerite and monazite. The absence of a plane corresponding to the cleavage direction is another mark of resemblance. Moreover, in hardness they are the same; in color very similar. Yet the actual identity of the species cannot be considered as settled without new crystallographic comparisons, or a chemical examination of turnerite. The trials by Mr. Children were too imperfect to be decisive against it; while they show that turnerite is not a titanate or silicate.

2. *Grahamite.*—Professor Henry Wurtz has proposed the name *Grahamite* (Report upon a Mineral Formation in West Virginia: New York,

1865) for the pitch-black Albertite-like mineral of Virginia described by J. P. Lesley (see this Jour., xxxvii, 149). Mr. Lesley took the ground that it was not true coal, and compared it to Albertite. We gather from Mr. Wurtz's Report the following facts. The vein occurs in Ritchie Co., in Carboniferous rocks, and occupies a *shrinkage* fissure. It is about 4½ feet wide; 2 inches outside are granular; the next 15 or 16 inches columnar and very lustrous; the middle, averaging 18 inches, though varying much, less columnar and less lustrous, and more resinous in fracture. He concludes that the fissure was filled by the exudation of the resinoid substance while it was in a pasty condition. G.=1·145. An analysis by Dr. J. Maier afforded C 76·45, H 7·82, O (with traces of N) 13·46, ashes 2·26=100. No action with cold or melted caustic potash, or boiling nitric or muriatic acid, or aqua regia; a brown solution with sulphuric acid. Wholly insoluble in alcohol; partly soluble in naphtha, benzole and ether; almost wholly so and readily in chloroform and sulphuret of carbon; mostly so, but slowly, in oil of turpentine. At 400° F. it begins to decrepitate, smoke and soften, and gives off water; above this, empyreumatic vapors and a pasty fusion internally, the exterior decomposing; and when drawn apart in this state it forms long delicate threads. Mr. Wurtz found that, under the same circumstances, *Albertite* might be drawn into threads.

The Report closes with suggestions as to methods of utilizing grahamite in the manufacture of illuminating oils and gas, a cement for sealing bottles (for which its indifference to acids and alkalies especially adapts it), translucent varnishes, lubricating compositions, etc.

3. *On the discovery of Corundum at the Emery mine, Chester, Mass.;* by Dr. C. T. Jackson. (From a letter to one of the Editors.)—At the middle of July last I found a perfect crystal of blue corundum or sapphire at the Chester mine. This crystal is among the specimens which I have arranged for the Emery company to send to the Paris Exposition. It is surrounded by magnesian carbonate of lime or crystallized dolomite. The form of the crystal is that of the double pyramid with six planes—like that figured by Dufrénoy on page 49, fig. 303; and it is three-tenths of an inch long.

4. *Note concerning the minerals of the Emery mine of Chester, Mass.;* by Prof. C. U. Shepard. (Communicated for this Journal.)—There are obviously two chloritic species in addition to the chloritoid found at Chester. The chloritoid analyzed by Dr. Jackson, of which an account was published in the last number of this Journal, is not the mineral[1] designated as such by me as occurring in the emery vein of the north mountain,[2] but the softer clinochlore. I at first supposed it to be corundophilite. The analysis refers to the same mineral as analyzed by Prof. J. L. Smith and quoted in the same number of the Journal. Both are analyses of a mineral whose crystallization, hardness and gravity, leave but little room to doubt that it is clinochlore; but the substance called the "fringe-rock," attached to both sides of a feldspathic vein of which I had said it was difficult to say "whether it is chlorite or corundophilite," cannot mineralogically belong to biotite, as Dr. Smith thinks, inasmuch

[1] That mineral is close to masonite or ottrelite.

[2] See p. 10 of my Report on the Emery mine of Chester. London, 1865.

as its hardness is only 1·5, whereas if biotite it should be from 2·5 to 3·0. Besides it is flexible and wholly inelastic. Nor can these differences arise from decomposition, for the mineral is perfectly fresh, shining and translucent. Its specific gravity is 2·76. Before the blowpipe it hardens, and then melts with the greatest difficulty upon thin edges into a pale bottle-green glass. It appears to me, therefore, to approach very closely to the Buncombe mineral found with sapphire, and called by me, corundophilite.[3]

Rutile.—This is rather frequent, in bright red, slender and much striated prisms, closely associated with diaspore and clinochlore, sometimes in reticulated aggregations.

Amherst College, Sept. 29, 1866.

5. *Laurite, a new mineral.*—Wöhler has discovered among the fine-grained platinum ore from Borneo, a new mineral, a sulphid of ruthenium and osmium, to which he has given the name *laurite*. It occurs in small grains of a dark iron-black color, and high luster. Most of the grains are true crystals, and Sartorius von Waltershausen has recognized the mineral to have the form of the regular octahedron, in some instances showing cubic, tetrahexahedral, and other planes. It has a distinct octahedral cleavage, is brittle and yields a dark gray powder on pulverization. Hardness, above that of quartz. Specific gravity, above 6 (6·99, Sartorius). When heated it decrepitates, and B.B. is infusible, giving first sulphurous and finally osmic acid fumes. Not acted upon by aqua regia or by fusion with bisulphate of potash. Fused with hydrate of potash in a silver crucible the mineral dissolves, yielding a green mass on cooling. Analysis gave, ruthenium 65·18, osmium 3·03, sulphur 31·79. The osmium was determined by loss, and Wöhler observes that the ruthenium was not entirely free from this substance, so that the percentage of ruthenium is given somewhat too high, while that of the osmium is correspondingly too low. The amount used for analysis (0·3145 gram), prevented a more accurate determination. Assuming the laurite to contain a sulphid of osmium analogous to osmic acid, the composition of the mineral may be represented by the formula

$$12(Ru_2S_3)+OsS_4=Ru\ 62{\cdot}88,\ Os\ 5{\cdot}00,\ S\ 32{\cdot}12,$$

or Ru_2S_3 91·8, OsS_4 8·2. This is the first instance of the occurrence of a natural sulphid in the group of platinum metals.—*Ann. Chem. Pharm.*, cxxxix, 116.

6. *Mt. Hood.*—The newspapers from the Pacific states contain accounts of two ascents of Mt. Hood during the past summer. From these, we glean but little information however, further than that the summit is accessible, for the accounts are very conflicting in their details, and some of the statements evidently very loosely made, while others are apparently wrong, which we must regret as the alleged facts may find their way into more enduring literature than the newspapers.

In July, 1864, the *Dalles* (Oregon) *Mountaineer* gave an account of a successful ascent made on the 17th of July of that year. The attempt

[3] The distinction between this mineral and the clinochlore of this locality is perfectly easy. The latter scratches gypsum with facility, whereas the former makes no impression on it.

was made by a party of four gentlemen of that place, three of whom gave out when near the summit, "but the fourth, Edward Ayres, persevered and succeeded in reaching the topmost pinnacle," which he represents as a "bare, rugged crag only large enough to stand upon." They found "a crater about 3000 ft. below the top, from which a sulphurous smoke ascended."

The present year, we have more detailed accounts of two other ascents. The first of these was made on the 26th of July, when "a party from Portland reached the summit after six hours travelling from the snow line." One of this party, Rev. H. K. Hines, gave an account in the Vancouver Register, of which we have seen only published extracts. He also speaks of "blue sulphurous smoke rising from fissures in the snow," and says that they "descended into the crater for a short distance but were stopped by a perpendicular wall fifty or sixty feet high." He describes the view as being especially grand, extending "from Mt. Rainer to Diamond Peak, a distance of 400 miles." (According to the maps, the actual distance between these peaks is but 260 miles.)

On the 20th of last August, another ascent was made by six gentlemen, one of whom, Prof. Alphonso Wood, has given a detailed account of the trip before the California Academy of Natural Sciences at their meeting of Sept. 3d. The report states that he found true glaciers on the flanks, with terminal and lateral moraines. He speaks of the slope having an angle of 45° for a mile and a half before reaching the crater, and 60° above that. The summit area he describes as "a crescent in shape, half a mile in length and three to fifty feet in width. It is a fearful place, as it is the imminent brow of a precipice on the north, sheer down not less than a vertical mile of bare, columnar rock." He too speaks of a crater, and we infer that it is very large and deep and mostly filled with snow except where melted by hot gases and steam. "On the west side of the crater is still an open abyss whence issue constantly volumes of strongly sulphurous smoke. That there is heat there, is evident from the immense depression in the snow about the place,—depressed not less than a thousand feet below the snows which fill to the brim the other portions of the ancient crater." He measured the various altitudes by observing the boiling point of water, and gives the following figures: "Summit of the Cascade Range and foot of Mt. Hood proper, 4,400 ft.; the limits of forest trees 9,000 ft.; highest limits of vegetation 11,000 ft.; summit of the mountain, 17,600 ft." The observed temperature of the boiling point he states at 180° F. on the summit, and from that deduces the last figures quoted, and closes by stating that we must consider this the highest measured point in the United States, if not in North America.

It seems highly probable however, that either his observations or his figures are very imperfect, and we are left still in the same uncertainty about the actual height of the peak that has existed heretofore. It is well known that the *estimated* height is stated in various works at from 7,710 ft. to 18,360 ft.

Those persons whose estimates seem entitled to consideration, (Lieut. Abbott, Dr. Newberry and others), state its height as less than that of Mt. Shasta, while the rough trigonometrical measurement made in 1860 by Dr. Vansant, gave the height at less than 12,000 ft. It is not easy

to see how Prof. Wood deduces his figures from the observations he gives. A boiling point of 180° F. represents a barometric pressure of 15·26 inches, which in that region, in the month of August, would probably represent an altitude of 18,350 feet or more. On Mt. Shasta, which is 300 miles farther south, the forest vegetation barely extends up 9,000 ft., the alleged height on Mt. Hood, where the alpine species are probably identical or similar.

It is noticeable how these accounts differ in other important particulars. One finds the summit a mere "pinnacle" only large enough to stand upon—another speaks of it as half a mile long. All agree that there is a crater, but one party finds it 3,000 ft. below the summit; we infer from the description of another that it is at the summit, or at least that a part of its rim forms the summit; one party descended into it a short distance, but finds a precipice fifty or sixty feet high; another speaks of a precipice of a mile vertical. The last observers find the crater nearly filled with snow, while but a few months ago the papers contained accounts of the mountain in active eruption. From all these we see we are still in doubt as to the actual height and condition of the peak; but since these ascents demonstrate that the summit is easily accessible, we hope soon to have more satisfactory observations. W. H. B.

7. *Alleged discovery of an ancient human skull in California.*—Accounts have recently been going the rounds of the press of the discovery of a human skull in or beneath certain volcanic deposits in California, which has attracted much attention from the various ages that have been assigned to it. The facts of the case, so far as they have reached us from authentic sources, are as follows. The skull in question is alleged to have been found at a depth of 153 feet, in a shaft sunk in the consolidated volcanic ash, known locally as "lava," near Angel's Camp, in Calavaras county. Five beds of this consolidated ash were passed through, separated by beds of gravel.

The skull was found by a miner, and it soon came into the hands of Prof. J. D. Whitney, state geologist of California, who visited the locality and investigated the matter as far as was then possible, but owing to the presence of water and the stoppage of work in the shaft, the examination was not fully satisfactory. He has made a preliminary statement before the California Academy of Natural Sciences, but defers any extended notice until the subject can be investigated with more completeness and accuracy. He thinks the skull was found in the position claimed, and will investigate the subject when the water is pumped out of the shaft and work resumed, which is expected to be done soon.

The precise age of the beds in question is as yet uncertain. In the geology of California, Prof. W. considers that the eruption of the great mass of volcanic materials on the western slope of the Sierra Nevada began in the Pliocene age, and that it continued into the Post-pliocene, and possibly to comparatively modern times. The alleged position of the skull is a lower one than any in which the remains of the mastodon have there been found, and therefore the question of its authenticity becomes a very important one; and when the more complete examination has been made, we will lay the results before the readers of the Journal.

W. H. B.

8. *On the discovery of the remains of a gigantic Dinosaur in the Cretaceous of New Jersey;* by E. D. Cope. (Proc. Acad. Nat. Sci. Philad., 1866, 275.)—Prof. Cope exibited the remains of a gigantic extinct Dinosaur, from the Cretaceous Green Sand of New Jersey. The bones were portions of the under jaw with teeth, portions of the scapular arch, including supposed clavicles, two humeri, left femur, and right tibia and fibula, with numerous phalanges, lumbar, sacral and caudal vertebræ, and numerous other elements in a fragmentary condition.

The animal was found by the workmen under the direction of J. C. Voorhees, superintendent of the West Jersey Marl Company's pits, about two miles south of Barnesboro, Gloucester Co., N. J.

The bones were taken from about twenty feet below the surface, in the top of the "chocolate" bed, which immediately underlies the green stratum which is of such value as a manure.

The discovery of this animal fills a hiatus in the Cretaceous fauna, revealing the carnivorous enemy of the great herbivorous Hadrosaurus, as the Dinodon was related to the Trachodon of the Nebraska beds; and the Megalosaurus to the Iguanodon of the European Wealden and Oolite.

In size this creature equalled the Megalosaurus Bucklandii, and with it and Dinodon, constituted the most formidable type of rapacious terrestrial vertebrates of which we have any knowledge. In its dentition and huge prehensile claws it resembled closely Megalosaurus; but the femur, resembling in its proximal regions more nearly that of the Iguanodon, indicated the probable existence of other equally important differences, and its pertaining to another genus. For this and the species the name of *Lælaps aquilunguis* was proposed.

The paper continues with descriptions of the mandible, femur, tibia, fibula, humerus, phalanges, vertebræ, etc.

9. *Exploration of the "Bad Lands" or "Mauvaises Terres" of the Upper Missouri region;* by Dr. F. V. Hayden.—Dr. Hayden has just returned from an extended exploring tour through the region of the Bad Lands. On his way out he left Fort Randall on the Missouri river, August 3d, with an escort of five soldiers, a six-mule team, one assistant, a guide and Indian interpreter, and an Indian as hunter—nine in all. The party went up the Niobrara, north side, as far as Rapid river, passed up that stream to its head, crossed over the divide to the south fork of White river, passed along the south side of White river to White Earth creek, about 100 miles north of Fort Laramie, at which point they were nearly south of the Bad Lands. From thence they traversed the whole of the Bad Lands and returned on the old Fort Pine road, thence on the south side of the Missouri to Fort Randall, having been absent fifty-two days. Dr. Hayden has made very extensive collections of fossils, including about fifty turtles, two of them of the largest size, nearly or quite perfect. The distance travelled on the way out from Fort Randall and back was 650 miles, and the specimens obtained were transported by land through that wild country for more than 300 miles. We hope soon to give a full account of the results of the exploration, which must be of great importance to geological science, coming from one so able and experienced in exploration, and so familiar with the whole Upper Missouri region.

10. *Post-Tertiary of Maine.*—Mr. John De Laski has recently sent us a number of species of Post-tertiary fossil shells from the island of North Haven, Penobscot bay (the next island above Vinalhaven), Maine. The collections were made at a height of 350 feet by barometer above the sea; they were from a layer about three feet below the surface in a bog. Among the species there are the following: *Mytilus edulis*, *Pecten Islandicus*, *Mya truncata*, *Mya arenaria*, *Saxicava arctica*, *Serripes Grœnlandicus*, *Astarte striata*, *Natica clausa*, *Buccinum undulatum* Mœl., *Buccinum flectrum* Stimp., var. *Packardi*, *Fusus tornatus* Gould.

The *Buccinum flectrum*, var. was found with other shells by Abraham Carver in digging a well about two miles farther north, at a depth of sixteen feet, in blue clay beneath gravel and sand. The assemblage of shells is like that inhabiting the Banks of Newfoundland and coast of Labrador.—Eds.

11. *Discovery of Mastodon remains at Cohoes, N. Y.* (In a letter to Prof. Dana from Robert Safely, Esq., dated Cohoes, Sept. 27, 1866.)—During some recent excavations made by the Harmony Mills Co., Cohoes, (about 1,000 feet below Cohoes Falls) for the foundation of a new mill, a number of pot-holes were discovered in an ancient bed of the Mohawk river, one of which contained the lower jaw of a Mastodon imbedded in peat and drift-wood. These "pot-holes" are worn in the Hudson river shale, about a hundred feet above the present bed of the Mohawk, and about a mile from where it enters the Hudson. The one containing the remains was about 250 feet from the south band of the Mohawk. The jaw, which was in an excellent state of preservation, measured about 28 inches in length and 22 in breadth between the condyles. On the right side there was one molar, and on the left side two, one of which was 4 inches, and the other 6½ inches in length.

12. *An addition to some notes "On a few of the fossiliferous localities of Livingston and Genesee counties, N. Y.," published in the January No. of this Journal;* by Henry A. Green.—In the black shales of the Portage Group, at the Buck Run locality in this town, in which I have heretofore found only a few fossil plants, I have recently discovered several bones, probably fish. The exposed portion of one of these measures four by five inches, and appearances indicate that when worked out it will not be less than seven inches across. Others appear to be as large in at least one direction, and several of them are an inch or more thick. One of the larger bones is somewhat broken; otherwise they appear to be well preserved, and when worked out will probably be of considerable interest.

At Batavia I have recently found another outcrop of the Marcellus limestone. The character of the rock is such that the fossils can be got out much easier and better than at Avon. This outcrop will, I think, furnish some fossils not heretofore found in this limestone.

Mt. Morris, Aug. 29th, 1866.

III. BOTANY AND ZOOLOGY.

1. DeCandolle, *Prodromus, Syst. Nat. Regni Vegetabilis.* Pars XV, sectio posterior, *sistens Euphorbiaceas.* Paris, 1862 and 1866, pp. 1286. —A part of this thick volume, p. 1–188, containing *Euphorbia* and its near allies, elaborated by Boissier, was issued four years ago. The rest of the *Euphorbiaceæ*, very ably worked out by J. Müller, is now published under the date of August last. The extent of the order has evidently confounded the calculations of the editor; for this thick volume of almost 1300 pages, occupied by the *Euphorbiaceæ*, does not comprise all that was origiually assigned to it, the *Bruxaceæ*, cited as synonymous on p. 1, being now excluded and referred to the ensuing volume. Under Dr. Müller's hands, the genera are arranged upon an intelligible system, under ten neatly characterized tribes, and the genera are not a little reduced. In consequence, *Phyllanthus*, with 438 species almost rivals *Euphorbia* itself; and *Croton*, received also almost in the widest sense, displays 453 species. *Acalypha*, the character of which remains unchanged, is here augmented to 215 species. It is a satisfaction to find, and it is a probable indication of superior excellence in this elaboration, that the number of species in relation to genera is unusually high, and that there are very few genera founded on single species. In view of facility of reference it is greatly to be desired,—and perhaps it is still not impossible by the aid of new title pages,—that these volumes 15 and 16 should be re-numbered and conformed to the actual state of the case; this huge second part of vol. 15 to be 16, the present 16, of which one fascicle has been issued, to be 17, and so on. The permanent advantage would much exceed any temporary inconvenience of the change.

A. G.

2. E. Boissier, *Icones Euphorbiarum, ou Figures de* 122 *Espêces du Genre Euphorbia, dessinèes et gravèes par* Heyland, etc. Paris, Victor Masson et fils. 1866. Royal fol.—Along with the volume of the Prodromus devoted to *Euphorbiaceæ*, we opportunely receive M. Boissier's magnificent folio, in which he has illustrated 122 selected species of the vast genus *Euphorbia*, one species to each plate. The plates, it will be seen are put upon stone, as well as drawn, by the veteran artist Heyland, and are in his best style,—a large part in outline, which is well adapted to this subject. Botanists are deeply indebted to M. Boissier for bringing out—evidently at much expense—this important work. A fair share of North American species are figured, viz: *E. acuta, angusta, zygophylloides, revoluta, florida, glyptosperma, hexagona, bilobata, exstipulata, Mercurialina, Wrightii, sphærosperma, trichotoma, dictyosperma, Texana, Peplidion, Rœmeriana*,—most of them species recently established either by Boissier or by Dr. Engelmann. Several pages of letter-press are occupied with remarks on the structure, classification, and geographical distribution of the genus. Linnæus described 64 species of *Euphorbia;* Boissier, in the Prodromus, including the recent supplement, has 717. A. G.

3. *On the young stages of a few Annelids;* by Alexander Agassiz. (Extracted from the Annals of the Lyceum of Natural History of New York, vol. viii, June, 1866.)—In this paper, which is prefaced by useful

observations on the habits and modes of collecting the young of these and other marine animals, the author has presented many new and valuable contributions to our knowledge of the development of several species of Annelids. Among these are species of *Planaria*, *Spirorbis*, *Terebella*, *Polydora*, *Nerine*, *Phyllodoce*, and *Nareda* (?). The latter is compared with the unknown larva described by Loven in 1842, which is, therefore, supposed to belong to some Nemertean genus like *Polia*. The paper concludes with brief remarks upon the Types of Development in Annelids. The work is illustrated by six wood-cut plates, containing fifty-six figures. A. E. V.

4. *Corals and Polyps of the North Pacific Exploring Expedition, with Descriptions of other Pacific Ocean species*, with four plates; by A. E. VERRILL. (Extracted from Proceedings of the Essex Institute, vols. iv and v.)—This pamphlet has been published in three parts, of which the two first have already been noticed in this Journal. Part III contains the *Madreporaria*, illustrated by two plates, one of which includes also a few species of *Actinidæ*. Three new genera, *Pachysammia*, *Cœlastrea* and *Cyclopora*, are described, and thirty-seven new species. Among the more interesting forms are a living *Eupsammia*, hitherto a tertiary genus, three species of *Stephanoseris*, an *Allopora* from California, and a *Diaseris*, with a figure of the living polyp.

5. *On the Polyps and Corals of Panama, with descriptions of new species;* by A. E. VERRILL. (From the Proceedings of the Boston Society of Natural History, April, 1866.)—This paper is prefaced by a comparison of the Polyp faunæ of the Atlantic and Pacific shores of Central America, showing a remarkable contrast—as had been previously determined for the Mollusca, Crustacea, and other classes, giving additional evidence of the improbability of oceanic communication across the Isthmus in recent geological times. Four new species of Alcyonaria and eight of Madreporaria are described; also a new genus, *Stephanocora*, belonging to the *Poritidæ*. All of the previously described species are mentioned, with their known distribution, and other observations.

6. *On the Polyps and Echinoderms of New England, with descriptions of new species;* by A. E. VERRILL. (Published and stitched with the preceding.)—In this paper special attention is devoted to the geographical distribution of these two classes on our coast, which is discussed in the introduction. The New England coast is considered as embracing a part of three marine faunæ: the Virginian south of Cape Cod, the Acadian along the eastern coast, and detached patches of the sub-arctic or Syrtensian fauna on deep-lying banks (like St. George's) off the coast. The theory is advanced that "an increase in *depth* of water has the same effect as increase in the *elevation* of land—that of causing a lower temperature, and consequently bringing northern animals down to lower latitudes than they can inhabit in shallower waters along the shore, thus giving rise to outlying patches of more northern faunæ far south of their proper limits on the coast."

The paper embraces a complete list, so far as known, of all the species in each fauna, with remarks on their distribution, their synonyms, &c. Two new species of *Sagartia* from near New Haven, and the two large species of *Asterias* from the eastern coast, not before sufficiently charac-

terized, are described. A new generic name, *Euryechinus*, is proposed for our common Sea-urchins, of which two species are recognized. A new genus, *Leptasterias*, is established for the small starfishes like *A. Mulleri* Sars, and a new species is described. For the *Psolus Fabricii* Lutken, a new genus, *Lophothuria*, is instituted.

This being the first attempt yet made to bring together and revise the synonymy of all our species of Echinoderms, it will doubtless be found very useful to those interested in the subject.

7. *Natural History of Animals;* by Prof. SANBORN TENNEY and Mrs. ABBY A. TENNEY. New York, 1866. (Chas. Scribner & Co.)—This little work is intended for beginners in natural history, and contains brief descriptions of the animals figured on the natural history tablets designed to accompany it. The book, however, is complete in itself, and contains five hundred beautifully executed wood-cuts, being a reprint of those in Tenney's Manual of Zoölogy. The attempt has been made in this book to free the subject from all technicalities, and to simplify it to the utmost extent. It would have been more generally useful, however, had the scientific names of the animals described or figured been given as well as the common names. Affording, as it does, figures and brief descriptions of large numbers of American animals, as well as some foreign, in a neat book at a low price, while it gives a general view of the animal kingdom in a popular form, it cannot fail to meet a want felt by many, for elementary works on American Natural History. A. E. V.

8. *Note on the Organisms of the Geysers of California;* by Prof. W. H. BREWER.—In the May number of this Journal (p. 392), in a letter to Prof. J. D. Dana, on the presence of living species in hot and saline waters of California, I misapprehended certain facts, relating to organisms in the hot waters of the geysers. I there stated that Mr. A. M. Edwards of New York had detected "animal as well as vegetable organisms in the specimens." Mr. Edwards writes me that he examined specimens collected "over hot stoves in water at 120·5° F.," and he states that he found a few remains of *Diatomaceæ*, of which he enumerated several species. No animal remains were found but such fragments (hairs) as might have been derived from outside sources. It is due Mr. Edwards that I make this correction, which arose from a misapprehension on my part of what species had been detected. In regard to the existence of other vegetable forms in waters of a higher temperature (200° F.), observed by myself, the facts were correctly given.

New Haven, Oct. 19, 1866.

IV. ASTRONOMY.

1. *Shooting Stars in August*, 1866.—(1.) *At Sherburne, N. Y.*—On the morning of the 10th of August the writer, with three assistants, saw *seventy-six* shooting stars between 12 and 1 o'clock. The floating clouds interfered seriously for a part of the hour. During the next hour we saw *thirty-eight*, notwithstanding the clouds, which were always numerous, and at times covered the sky. On the next morning the sky was clear, and watching alone I saw 51 shooting stars in the hour between one and two o'clock. The number seemed to diminish toward morning.

(2.) *At Germantown, Pa.*—Mr. B. V. Marsh and Mr. R. M. Gummere watched with the following results.

Aug. 9th, between $8^h\ 40^m$ and 9^h P.M. Mr. Marsh alone saw 7 meteors, 5 conformable. Aug. 10th, between $8^h\ 47^m$ and $9^h\ 7^m$ P.M., 10 meteors. The weather was very fine. There was perhaps a slight haziness, but very small stars were very distinct. The following table exhibits the results of observation on the morning of the 11th.

	By Mr. B. V. Marsh.			By Mr. R. M. Grummere.		
	Conformable.	Non-conformable.	Total.	Conformable.	Non-conformable.	Total.
h m h m						
From 0 0 to 0 15 A.M.	14	1	15			22
0 15 " 0 30 "	15	3	18			19
0 30 " 0 45 "	8	2	10	6	2	8
0 45 " 1 0 "	13	3	16	24	3	27
1 0 " 1 15 "	14	4	18	13	3	16
1 15 " 1 30 "	8	5	13	13	4	17
1 30 " 1 45 "	10	3	13	15	3	18
1 45 " 2 0 "	11	3	14	19	2	21
2 0 " 2 15 "	10	2	12	16	0	16
	103	26	129			164
	Average, 57·3 meteors per hour.			Average, 72·9 meteors per hour.		

The average magnitude was decidedly below that of former years. Only a few left persistent trains, and there were none of very great splendor. The weather was clear and circumstances were altogether favorable. The two observers were independent of each other.

(3.) *At Westchester, Pa.*—J. H. Worrall, Ph.D., watched on two mornings with these results:

	Aug. 11.	Aug. 12.
From 2^h to $2\frac{1}{2}^h$ A.M.	25 meteors.	16
" $2\frac{1}{2}$ to 3 "	18 "	15
" 3 to $3\frac{1}{2}$ "	19 "	21
" $3\frac{1}{2}$ to 4 "	13 "	11
Total,	75	63

The sky was perfectly clear from all haze on the first night. The clouds on the second night obscured the south and southeast to 30° high. The star ϵ Ursæ Minoris was distinctly visible. During both nights the meteors emanated from a point near the constellation Perseus. They were irregular in the intervals of their appearance. Sometimes several would follow in quick succession, and then several minutes would elapse without one being seen.

(4.) *At Natick, Mass.*—Mr. F. W. Russell, on the evening of Aug. 6th, saw in $1\frac{1}{2}$ hours, 10 meteors (2 conf. and 8 unconf.); on Aug. 7th in 1 hour, 20 meteors (7 conf. and 13 unconf.); and Aug. 8th in one hour, 24 meteors (14 conf. and 10 unconf.). It rained on the 9th. On the night of the 10th–11th he saw as follows, watching alone:

9^h–10^h	16 conf.	8 unconf.	Total 24			
10–11	24 "	10 "	" 34			
11–12	44 "	11 "	" 55	With trains,		13
12– 1	59 "	9 "	" 68	"		13
1– 2	76 "	10 "	" 86	"		10
2– 3	86 "	11 "	" 97	"		21
Total in 6 hours,	305	59	364			57

(5.) *At sea near Martha's Vineyard.*—Mr. Isaac Pierson saw, while entering Martha's Vineyard Sound, 100 meteors in two hours between $9^h\ 15^m$ and $11^h\ 30^m$ P. M. of Friday the 10th of August—omitting the quarter hour from $10^h\ 45^m$ to 11^h. During the first half hour the sky was about one-eighth covered with clouds.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *National Academy of Sciences.*—The sixth stated session of the National Academy of Sciences was held at Northampton, Mass., on the 7th of August last. The following is a list of the papers read:

(1.) On a photometric method, by Prof. O. N. Rood.

(2.) On a normal map of the solar spectrum, by Prof. Wolcott Gibbs.

(3.) On traces of glaciers under the tropics, by Prof. Louis Agassiz.

(4.) On the secular acceleration of the moon's mean motion, by John N. Stockwell; read by Dr. B. A. Gould.

(5.) On the origin of solar heat, by Prof. B. Peirce.

(6.) On the morphological value and relations of the human hand, by Dr. Burt G. Wilder.

(7.) On the correlation of gravity and temperature, by Pliny E. Chase.

(8.) On the grounds of analogy between linguistic science and the physical sciences, by Prof. Wm. D. Whitney.

(9.) On the limitation of homologies, by Prof. Louis Agassiz.

(10.) On a new method of optical analysis, by Prof. Wolcott Gibbs.

(11.) On recent soundings in the Gulf Stream, by Mr. Henry Mitchell, U. S. Coast Survey.

(12.) On repeated linear substitutions, by J. E. Oliver.

(13.) On the metrical system of weights and measures, by Samuel B. Ruggles.

(14.) On some points in the geological structure of southern Minnesota, with reference also to the period of denudation of the older formations, by Prof. James Hall.

(15.) A new theory of planetary motion, by Prof. T. Strong.

(16.) On the linear evaluation of surd forms, by Prof. James Watson.

(17.) On the study of young animals, and its bearing upon the progress of paleontology and zoölogy, by Alex. Agassiz; read by Prof. Louis Agassiz.

(18.) On a remarkable rainbow, by Prof. Peirce.

(19.) An investigation in regard to sound in its economical application, by Prof. Joseph Henry.

(20.) On the geographical distribution of fishes in the waters of the Amazon, by Prof. Louis Agassiz.

(21.) On the stature of American soldiers, by Dr. B. A. Gould.

(22.) On the influence of the hour of the day on the height obtained by barometric measurements, by Prof. A. Guyot.

(23.) On astronomical photography, by Lewis M. Rutherfurd.

(24.) On the reduction of photographic observations, with a determination of the position of the Pleiades from photographs by Mr. Rutherfurd, by Dr. B. A. Gould.

(25.) On a table for facilitating the conversion of longitude and latitude into right ascension and declination, by Wm. Ferrel.

(26.) On the Nephila plumipes or silk spider of South Carolina, by Dr. BURT G. WILDER.

Prof. J. P. LESLEY read a biographical notice of the late Prof. Edward Hitchcock.

2. *Meteorite in Hungary of June*, 1866; by JOSEPH SZABO, Professor of Mineralogy and Geology at the University of Pest, and Reporter of the Committee for mathematical and physical sciences at the Academy. (From a letter to Prof. HENRY, Smithsonian Institution, dated Pest, July 31, 1866.)—A fall of a meteorite took place in the N.E of Hungary, the 9th of June 1866 at 5 P. M. The locality is a small village called *Knyahinya*, inhabited by Rusznyaks, about one Austrian mile from the village Gr. Berezna, and five miles from Ungvár, the capitol of the county of Ung; the place above named is on the boundary of the two counties Ung, and Zemplin, and N.N.W. from Ungvár. We call it therefore "the meteorite from *Knyahinya* N.W. of Hungary."

The luminous meteor was first observed in the neighborhood of Kaschan, and was seen to proceed in an eastern direction. The people nearer to the east say that there was a violent detonation, accompanied by a small cloud, and by the fall of several pieces of stone. A Jew, not far from whom a piece fell, after having taken it into his hands, felt it *cold*.

About seventy pieces have been found; the largest is said to have a weight of 36 Austrian pounds, but it was broken on the spot by the finders, so that the largest piece of it weighs now about fourteen pounds. It became the property of a naturalist in Pest. I have received a notice of three pieces, weighing each about 200 Vienna pounds; but this fact must be verified.

The Hungarian Academy of Sciences at Pest has taken proper measures in order to save a part of the fallen pieces for the interest of science, and it has become, through the coöperation of disinterested and zealous persons, the proprietor of nearly half of them. It has been determined to send some of them to different scientific institutions in Europe and in the United States, and thus also to the mineralogical department entrusted to your care. I have the honor to announce to you, that a piece in a perfect state of preservation will be transmitted to you about the month of September, after the examination of some of its physical properties is completed. Dr. Than, professor of chemistry at the university, is occupied in making a chemical analysis of it.

3. *Lyceum of Natural History, New York.*—We mentioned in the July number of this Journal, the destruction by fire of the building of the Lyceum of Natural History of New York. We are informed that, although the Lyceum suffered great and, in some respects, irreparable loss, in the destruction, for the most part, of the zoological, mineralogical and paleontological portions of its collection, yet its very extensive and valuable herbarium—(containing plants collected and determined by such eminent botanists as Torrey, Gray, Halsey, the two Carys, Bulkley, Browne and others) was preserved intact. So also was its Library, comprising series of the Transactions of nearly all the learned societies of Europe and America, and the stock of the Lyceum's own "Annals." All these had been removed from the Medical College a year or two previous to the fire,—and by the kindness of the Directors of the Mercantile Library Association, had been permitted to occupy a place upon its shelves.

To repair, as far as possible, the loss which the Lyceum has sustained, many of its members stand ready to give large private collections in the several departments of natural history, whenever a sufficient and safe building shall be secured for their reception. For this, they look to the known liberality and public spirit of the wealthy citizens of New York, and believe they will not look in vain.

4. *Gifts of Mr. George Peabody to Science.*—Mr. George Peabody has recently given $150,000 to Harvard College for the establishment of a Museum of American Archæology and Ethnology, and the same amount to Yale College for a Museum of Natural History.

5. *British Association.*—The meeting of the British Association for the current year was held at Nottingham during the week commencing Aug. 22d, Mr. Groves being the President. Dundee was appointed as the place of meeting for the next year.

6. *Dr. Krantz.*—Dr. Krantz has purchased the extensive collection of fossils and minerals belonging to the Comptoir Minéralogique and Géologique of the late Mr. Louis Sæmann of Paris, and is about adding them to his own great establishment for the sale of specimens at Bonn on the Rhine.

OBITUARY.

Mr. Edmund Blunt, first Assistant upon the United States Coast Survey, died on the 2d of September last. Mr. Blunt was a son of Edmund M. Blunt, author of the American Coast Pilot, and was born in Newburyport in Nov., 1799. In early life he manifested a great interest in hydrographical pursuits. In 1816 he made a survey of the harbor of New York, and this was the first survey of that harbor ever made, for the chart of Des Barras must be considered simply as a sketch. In 1819 and 1820 he made the first surveys of the Bahama Banks, and the shoals of George's and Nantucket. In 1824 he surveyed the entrance of New York harbor from Barnegat to Fire Island. In 1825 and 1826 he run a line of levels from the river St. Juan to the Pacific Ocean for the purpose of building a canal on the Nicaragua route. From 1827 to 1830, as a private enterprise, he surveyed Long Island Sound from New York to Montauk Point, the government up to that time having done nothing whatever to develop a knowledge of the coast of the United States. On the organization of the U. S. Coast Survey, Mr. Blunt was appointed first assistant, which position he held until his death.

He triangulated Long Island, the shores of Connecticut and Rhode Island, ending with a base of verification near Providence, R. I. He also triangulated Delaware Bay from Philadelphia to the capes; also the Chesapeake from its head to the capes; and the Hudson river to a point above Troy. In 1855 and 1856 he furnished the points to determine the exterior line of New York harbor, which has done so much for its preservation.

Before and while on the Coast Survey, his attention was directed to the inferiority of the lights in the American light-houses, and he was mainly instrumental in introducing the Fresnel system into our country; a system which has contributed so much to the safety of navigation. Mr. Blunt was a mechanic of great inventive power. The dividing engine, built from his plan and under his direction, is an evidence of his knowl-

edge. Mr. Blunt was a true American, always solicitous for the honor and advancement of his country; and when the late rebellion broke out, he devoted all his energies to the support of the government.

A. A. Gould.—Dr. Augustus A. Gould died in Boston, Sept 15th, at 5^h 30^m A. M., of cholera, after an illness of only a few hours. Dr. Gould was the son of Deacon N. D. Gould, late of Boston. He was born in New Ipswich, N. H., April 23, 1805, and graduated at Harvard College in 1825. He pursued the study of medicine with Drs. James Jackson and Walter Channing, and immediately thereafter commenced practice in Boston. Although constantly engaged in the active duties of his profession, science was the leading passion of his life, and by zealously devoting his leisure moments in the intervals of business, and, as he has expressed it, "hours stolen from sleep" to his favorite studies he has made his name widely known as a scientific student and author by many valuable contributions. He became very early one of the most active members of the Boston Society of Natural History, and has continued his interest and coöperation in it until the day of his death, holding then the office of vice-president, a position he has filled for several years. The day before his death he spent a long time at the Society rooms, probably the last business that he did away from home. He was also a Fellow of the American Academy of Arts and Sciences; of the American Philosophical Society; of the National Academy of Science; and two years ago was unanimously elected President of the Massachusetts Medical Society. Many of his contributions to science have been published in the Proceedings and Memoirs of these societies. Many of his conchological papers, especially, have appeared in the Journal and Proceedings of the Boston Society of Natural History. In 1841 he published his Report on the Invertebrates of Massachusetts, an appropriation for that purpose having been made by the State. This, being one of the pioneer works on the subject in this country, is remarkable for its accuracy and general usefulness, and has always been one of the standard works on American conchology, that part of the book relating to the shells being the most voluminous and complete, and each species being well figured from drawings made mostly by the author's hand. The Legislature of 1865 made an appropriation of $4,000 to republish this work, and for several months he has been engaged in revising and enlarging it for that purpose. It is to be hoped that this labor was so nearly completed as to admit of the issue of the work at an early day. He published in connection with Prof. Agassiz, the "Principles of Zoölogy," in 1848. This work has become well known and widely circulated. In 1846 he was employed by the United States government to write the Report upon the Shells of the Wilkes Exploring Expedition, and contributed a quarto volume, with a folio atlas of plates, toward the history of that voyage. In 1863 he published, under the title of "Otia Conchologica," all the original descriptions of new species of shells published in his various works, with notes on changes in their nomenclature. His extensive collection of shells was recently purchased by the Boston Society.

His contributions to medical science are also numerous. In the department of vital statistics he was eminent among American students of that subject. He contributed to nearly every volume of the Registrar-General of Massachusetts papers of great labor and value.

He was a man of wide and general culture, and stood in the highest rank of professional eminence. His influence in promoting and diffusing the study of Natural History will never cease to be felt in America.

R. W. Gibbes.—Robert Wilson Gibbes, of Columbia, South Carolina, died in that city near the close of September. Dr. Gibbes had been one of the most active men of science in the Southern States. He was born in Charleston, S. C., July 8th, 1809. His chief scientific researches were directed toward the description of organic remains from his native State, and his memoirs include a "Monograph on the fossil Squalidæ of the United States;" a "Memoir on the fossil genus Basilosaurus," and another on "Mosasaurus and the three allied new genera, Holocodus, Conosaurus, and Amphorosteus," the first two published in the Journal of the Academy of Sciences of Philadelphia, and the last in the Smithsonian Contributions to Knowledge, vol. vii, Nov. 1849. He was also the author of important papers on medical subjects, and of a "Documentary History of the American Revolution," in three volumes. His house, with its library and collections, was destroyed during the passage of Gen. Sherman's army through South Carolina near the close of the late rebellion, which he made the reason for his formal withdrawal from the American Association for the Advancement of Science at their late session in Buffalo.

Louis Sæmann.—Mr. Sæmann died suddenly at Paris on the 23d of August last. He had been for many years proprietor of a large establishment in that city for the sale of minerals and fossils, and by his fidelity in all business transactions and the urbanity of his manners had won the confidence and regard of all who came into contact with him. He also commanded their respect as a man of science, for he was an excellent mineralogist, geologist, and paleontologist, and had published valuable papers in each of these departments. After extensive tours in Europe he came to this country in 1847 and spent nearly a year in making extensive collections in geology and mineralogy. Prof. Dana acknowledges his indebtedness to Mr. Sæmann in the Preface of the last edition of his Mineralogy for the facts and suggestions which he had communicated; and in the preparation of the new edition, now in progress, his correspondence has been of like service and value.

Major Robert Kennicutt.—It is with great regret that we have to announce the death of Major Kennicutt, chief of explorations of the Russian-American Telegraph Co. The news came first by telegraph. The following brief account of the circumstances of the event we gather from a letter from Mr. Charles Pease, one of the party under his command, to his friends at Cleveland, Ohio. Major Kennicutt, with a corps of naturalists, under the patronage of the Overland Telegraph Co., had made his way by land from San Francisco to the North Pacific. After their arrival at St. Michael's he met with many disappointments and failures, and though they were perhaps unavoidable, the effect upon him was very disastrous. They seemed to overcome him more than the hardships and sufferings he had previously undergone. He complained much of dizziness and strange distress in his head. On the morning of the 13th of May he was found by two of the party not more than two hundred yards from the Russian fort Nulato, lying upon the ground, dead. An open compass lay beside him; apparently he had been taking bearings and had

fallen suddenly. With very great difficulty the remains were brought on by his friends to Fort St. Michael's, from which it is expected they will be transported to this country. The death of this young, enthusiastic and persevering explorer is a great loss to the cause of science. We trust in another number to give a sketch of his brief career.

VI. MISCELLANEOUS BIBLIOGRAPHY.

1. *Smithsonian Institution: Report of the Secretary to the Board of Regents, January*, 1866 (being a part of the annual volume for 1865). Washington, D. C., 1866. 8vo, pp. 74.—The Secretary gives in this Report his usual summary of the various and varied operations of the Smithsonian Institution for the year 1865. A review is given of the circumstances resulting in the destruction of a portion of the building and contents by fire in 1864. The fire had its origin in an unpardonable blunder of a workman who opened a communication for a stove-pipe into a "furring space," mistaking it for a chimney flue. The products of combustion were thus brought among the roof-timbers and ultimately set them on fire. Nothing could better illustrate the importance of making such buildings fire proof. The very roof which burned the building was made in wood (although covered with slates), from motives of economy, in the original construction. The loss of the manuscript correspondence from the office of the Secretary, of his private manuscripts and records of subjects of science, of memoirs accepted for publication, copies of the Smithsonian reports, stereotype plates, wood-cuts and steel plates, seven years records of the force and direction of the wind in the anemometer tower, are the chief sources of regret in the results of the fire, although the loss of the apparatus presented by Dr. Hare, of Priestley's lens with which the discovery of oxygen was made, and many interesting personal memorials of Smithson is irreparable. The plan adopted for the reconstruction of the portions of the building destroyed contemplates the use of only fire-proof materials. The outlay required to meet these expensive restorations will be so managed as not to impair the general operations of the institution, but will prevent the undertaking of new enterprises.

The current income of the Institution for 1865 was $84,956.37, of which $32,115.97 were expended for the maintainance of the establishment and for carrying on all the operations of the Institution, leaving the balance available for reconstruction. The fund has received an addition from the death of an annuitant of Smithson, which amounts to $54,165.38, invested in U. S. $7\frac{3}{10}$ per cent bonds, the increase of which will hereafter be available for the general fund.

As the government of the United States is largely debtor to the Smithsonian Institution for labor bestowed on its scientific work, and much the larger part of the building and its maintainance are for the conservation of the public collections, there can be no reasonable doubt that the cost of restoring the building ought to be paid out of the public treasury, including the arrearages already paid by the fund, thus restoring the full value of Smithson's bequest to its legitimate objects, "the increase and diffusion of knowledge among men." The jealous care and watchful integrity and courage with which the distinguished Secretary has ever guarded the main object of the trust, often against powerful official and

legislative influences, is worthy of all praise, and receives the commendation of investigators of truth everywhere.

The volume also contains a number of valuable memoirs of considerable length either prepared for the Report, or cited from foreign Journals, including a paper on the Aurora Borealis, by ELIAS LOOMIS; on the Senses of Feeling and Smell; on Electro-physiology, by MATTEUCCI; on Lacustrine Constructions on Lake Neuchatel, with many figures, by E. DESOR; Systematic Review of the Class of Birds, by Prof. LILLJEBERG, etc.

2. *History of the Atlantic Telegraph;* by HENRY M. FIELD, D.D.—This work is a vivid sketch of the entire history of transatlantic telegraphy, by a brother of Mr. Cyrus W. Field, whose name has become forever identified with the inception and progress of that great enterprise which, after surmounting vast difficulties, has at last united Europe and America.

The new cable encloses and insulates a copper conductor, formed, like the old one, of seven strands of the purest copper, the weight of which is three hundred pounds to the mile; the old one weighed but one hundred and seven pounds. This conductor was first imbedded in "Chatterton's compound," a preparation impervous to water, and then covered with four layers of gutta percha, laid on alternately with four thin layers of Chatterton's compound. The old cable had but three coatings of gutta percha with nothing between. Its entire insulation weighed but two hundred and sixty-one pounds to the mile, while that of the new one weighed four hundred pounds. The armor which gave the requisite strength to the new cable consisted of ten solid wires of the best soft steel, each wire being surrounded separately with five strands of Manilla yarn saturated with a preservative compound. This armor was laid on spirally about the core, from which it was also separated by a padding of hemp saturated with the same preservative mixture. The cable thus constructed combined the flexibility of a ship's rope with the strength of an iron chain, while its greater volume gave it an important advantage in buoyancy over the old one, causing it to sink slowly and avoiding the danger of fracture from a sudden lurch of the ship, which in 1857 caused the parting of the cable of that year. The breaking strain of the new cable was seven tons and fifteen cwt.; that of the old one was three tons and five cwt. The contract strain of the new cable was eleven times its weight per mile in water, the greatest depth of water to be passed being but two and a half miles.

Capt. (now Sir James) Anderson, who was in command of the Great Eastern, is a navigator whose attainments in his profession peculiarly fitted him for the difficult task of laying the cable, and the yet more difficult one of recovering that of 1865 from its resting place in mid-ocean. The satisfactory discovery of perfect insulation in this cable after being a year submerged, is not the least noteworthy feature of the series of successes which has been rewarded with knighthood conferred on Anderson, Thompson, and their companions, by the Queen of England.

3. *Elements of Quaternions;* by the late Sir WILLIAM ROWAN HAMILTON; edited by his son, William Edwin Hamilton. 8vo, pp. lx, and 762. London. (Longmans, Green & Co.)—This volume had been promised and its appearance was awaited with interest by mathematicians for some

time previous to the lamented death of its distinguished author. It was not quite finished when he died, and the son has published it as left by the father. The Board of Trinity College, Dublin, has generously assumed the expense of the publication.

The first development of the Calculus of Quaternions was communicated in 1843 by Hamilton to the Royal Irish Academy. Ten years later a more complete and more geometrical presentation of this remarkable addition to Mathematical science was published in a thick octavo volume entitled *Lectures on Quaternions* (Dublin 1853). The later years of the author's life have been spent upon the present volume, which covers the same ground as the *lectures* and yet can hardly be regarded as a second edition of them.

In the hands of the author this Calculus had most wonderful scope and power. Whether it is to be the great instrument which future mathematicians will employ in subduing new realms of the Physical and exact Sciences, and in developing the riches of the old, it is probably premature to assert. Those capable of judging believe that its power and importance cannot be overestimated, and that it is destined to change the whole character of the higher mathematics.

A student should read this volume rather than any presentation of the subject that may be given by another mind. It requires a previous knowledge of the elementary mathematics including the more recent developments in Geometry as given in *Salmon's Conic Sections* or *Chasles' Geomètrie Superieure*, the Geometry of three dimensions, Differential and Integral Calculus, and the applications of the calculus to geometry. Some portions of the volume, especially the later pages, imply also a knowledge of Analytical and Celestial Mechanics.

The question is often asked, "What is a Quaternion?" A brief explanation may serve to the curious as a partial answer. We sometimes say that there are in geometry three kinds of magnitudes, volumes, surfaces, and lengths. The first are increased by the product of three linear factors, the second by the product of two linear factors, and the third by linear terms. If now we extend this series downward, we have a fourth class of magnitudes which are severally measured by the ratio of one line to another, that is, by a zero number of factors. Such are the circular functions, and angles. To this class belongs the Quaternion. Like the sine and the tangent it is the quotient of one line divided by another. But the lines are considered, in this instance, to have not only length but also direction in space. There enters into the conception of the quaternion, 1st, the relative length of the two lines; 2d, the angle which they make with each other; and 3d, the direction of their plane. The first two of these are each determined by a single condition, the third by two conditions. Hence there are four arbitrary quantities in any algebraic expression of this quotient, from which comes the name *quaternion*. The power of this calculus regarded as an extension of algebra, depends upon its use of the imaginary expressions to denote directions. H. A. N.

4. *A Preliminary Report of the Texas Geological Survey, together with Agricultural observations and an outline of the Mineral Deposits of the State;* by S. B. Buckley. 86 pp., 8vo. Austin, Texas, 1866.—Mr. Buckley was assistant to Dr. Moore, who had the charge of the geological survey of the state previous to 1861 when the survey was sus-

pended, and before that to Dr. B. F. Shumard. He presents in this report results of the observations made in the course of the preceding survey, results which show how much remains to be done before we have any just idea of the geology of the great state. The pamphlet consists largely of miscellaneous information on general geology, agricultural topics, and other matters of economical interest, together with an appendix containing descriptions of some Texan grasses regarded as new species.

5. *On the Geology of the Key of Sombrero, W. I.;* by ALEXIS A. JULIEN, Assistant in the School of Mines, Columbia College, New York. 28 pp. 8vo, with two plates. (From the Annals of the Lyc. of N. York, vol. viii.)—Mr. Julien has already published in this Journal on the minerals of the guano-bearing coral island Sombrero. This memoir gives a geological account of the structure of the coral island itself, and of the changes it has undergone. From the study of the several beds of coral rock and the surface features, he concludes that there were both subsidences and elevations in the progress of the island, and makes out eight elevations, and ten subsidences. The eighth elevation was to a height of 160 feet, after which there was a subsidence of about 40 feet, and another of 80 feet. The length of the island is about one mile; the breadth 200 to 1500 feet; the height over the southern third about 40 feet above the sea, and over the central and northern, half this. For the evidences of the changes of level and other points connected with the deposits of guano we refer to the memoir.

6. *Memoir on the Island of Navassa, W. I.;* by EUGENE GAUSSOIN, Mining Engineer and Metallurgist. 32 pp. 8vo. Baltimore, 1866. Also, *The Island of Navassa illustrated*—folio. Idem.—The island of Navassa is, like Sombrero, an elevated coral island, affording guano. Mr. Gaussoin has given an interesting description of this island, and illustrated the subject with six large folio chromo-lithographic plates, containing various views of the island, and making an elegant atlas. This island is in the Caribbean sea, in 18° 25′ N. and 75° 5′ W., 33 miles southwest of Hayti. The greatest height is 300 feet. It has perpendicular cliffs of compact coral-made limestone on all sides, and these cliffs are penetrated, as usual in such cases, by numerous caverns. The summit is lowest at center, atoll-like; and there is abundant evidence that the island is an elevated atoll, whose sides have been worn off into cliffs by the battering waves. Whether there were successive stages in the elevation remains to be ascertained. The change of limestone to phosphate through the presence of guano deposits is abundantly exemplified in many places on this island, as it is on Sombrero. The freshwater of the island the author remarks is simply the water of the rains which descends below the surface and rests on the subjacent saltwater; and he observes that in digging for water it is important not to go below lowtide level, as the water then becomes brackish. In this view he concurs with R. J. Nelson, the author of a memoir on the geology of the Bermudas. The volume closes with analyses of the guano of the island.

7. *Peat and its uses, as fertilizer and fuel;* by SAMUEL W. JOHNSON, Prof. of Analyt. and Agricult. Chem., Yale College. 168 pp. 12mo. New York, 1866. (O. Judd & Co.)—This little manual contains more information upon the subject of which it treats than any other work with which

we are acquainted, and it is eminently practical in the arrangement and treatment of the topics embraced. It is divided into three parts, the first treating of the origin, varieties and chemical characters of peat; the second, of its agricultural uses, including muck; the third, of its uses for fuel.

Several years since the same author, as chemist to the State Agricultural Society of Connecticut made an extensive investigation into the chemical characters and agricultural uses of these substances, and his Report, published by the society, is well known. Since the publication of that report, additional investigations have been made, one series of which possess a peculiar interest to the agriculturist, as they relate especially to the action of various composting materials upon peat. Without reviewing here the details of these experiments, which are given at length in the work (p. 77), it is sufficient to state the following as among the results; that the admixture of ashes, carbonate of lime, slacked lime, and Peruvian guano, tended to greatly increase the amount of plant food in decomposing peat, the crops in extreme cases being augmented thirteen fold over the production from pure peat, and, without any admixture containing nitrogen, eleven fold.

In regard to its value for fuel, a subject now attracting so much attention, we have here data afforded by the various processes engaged in its preparation in this country and in Europe. The great question of its profitableness now remains to be solved by the many experimenters in this branch of industry, many of whom need the data and facts here brought together for a more intelligent direction of their labors. This is especially the case, as regards the comparative heating effects of peat and coal, upon which popular opinion is so erroneous. W. H. B.

8. *Recherches sur l'origine des Roches*, par Delesse, Ingénieur en chef des Mines, &c. 74 pp. 8vo. Paris, 1865. (F. Savy.)—Delesse has written much upon the origin of rocks, and whatever comes from his pen is deserving of study. Many important points are discussed in this memoir.

9. *Geology and Minerals: a report of Explorations in the Mineral Regions of Minnesota during the years* 1848, 1859 *and* 1864, by Col. Charles Whittlesey. 54 pp. 8vo. Cleveland, 1866. Printed by order of the General Assembly.—We barely announce this memoir, as one containing many facts of value on the subjects of the surface features of the region mentioned, the phenomena of drift, and the distribution and features of some of the rocks.

10. *Carte Géologique du Department de la Seine*, publiée d'apres les orders de M. LeBaron G. E. Haussmann, Senateur Préfet de la Seine, conformément a la déliberation de la Commission departmentale et exécutée sur la carte topographique, gravée sons la direction de M. l'ingenieurs en chef des Ponte et Chaussees, par M. Delesse, Ingenieur des Mines du department de la Seine. 1865.—We defer our notice of this very beautiful and complete geological chart to another number.

11. *Chambers's Encyclopedia.*—Parts 107, 108, 109, 110 of this excellent Encyclopedia have just been issued by Messrs. J. P. Lippincott & Co., Philadelphia, carrying the work to the word "Syntax."

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